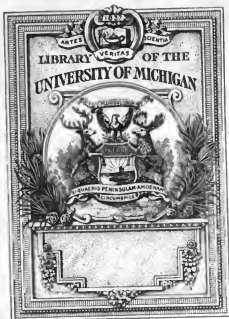
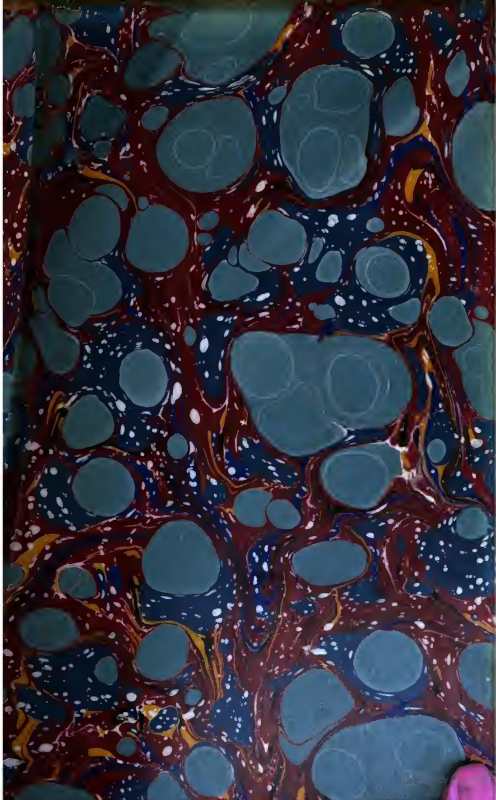


A 573169





Astron.

Obs.

QB

.A8

THE
ASTRONOMICAL REGISTER:

A MEDIUM OF COMMUNICATION
FOR AMATEUR OBSERVERS, AND ALL OTHERS INTERESTED IN THE
SCIENCE OF ASTRONOMY.

VOL. XVI.
NOS. 181 TO 192: JANUARY TO DECEMBER, 1878.

LONDON:
J. D. POTTER, 31, POULTRY, & 11, KING STREET, TOWER HILL.
1879.

LONDON :
ALABASTER AND PASSMORE, 31, LITTLE BRITAIN, E.C.
1879.

THE ASTRONOMICAL REGISTER.

INDEX TO VOLUME XVI.

Abney, Photography of the Least Refrangible End of the Solar Spectrum, 115

Adams, A Remarkable Property in the Analytical Expression for the constant term of the Moon's Horizontal Parallax, 168

Airy, The Measurement of the Transit of Venus Photographs, 173

— The Transit of Mercury, 139
a CETI, a Probable Radiant Point, 106

a Ursæ Majoris, Changes of Colour of, 79

Asteroids, Elements of the, 17

ASTRONOMICAL OCCURRENCES :—

Jan., 24	July, 186
Feb., 56	Aug., 212
Mar., 82	Sept., 236
April, 110	Oct., 260
May, 134	Nov., 284
June, 162	Dec., 310

Backhouse, on Comets and Meteor Radiant Points, 280

— on Jupiter, 255

— on Linné, 105

Bacon, Roger, 219, 255

β Corvi, 158

Birt, Lunar Objects for Observation :—

Jan., 26	July, 184
Feb., 59	Aug., 210
Mar., 85	Sept., 234
April, 108	Oct., 258
May, 137	Nov., 282
June, 164	Dec., 312

Boys, An Electric Clock, 4

Brett, The Transit of Mercury, 143

Burnham, on a New Nebula, 157

— on ω^1 Cancri, 77

— on Polaris, 107

— on ζ^3 Virginis, 130

CERES, EPHEMERIS OF, FOR OCT., 258

Chambers, Swift's Comet, Elements of, 258

— Tempel's Comet of 1878, Elements and Ephemeris of, 210, 257

— The Transit of Mercury, 143

Christie, Bright Lines in the Solar Spectrum, 178

— The Specular Reflection of Venus, 37

— The Transit of Venus, 141

Circle, A Combined Position and Setting, 103

Comet I., 1870, and the Perseids, 308

— Encke's, 282

— Swift's, 258

— Tempel's of 1873, 210, 257

— The Great of 1861, 208

— A Theory of, 78, 131

Comets and Meteor Radiant Points, 280

Common, The Satellites of Mars, 34

Corona, The Gaseous Part of the, 281

DE BOZ, on Miraculous Vision, 17

De La Rue, Solar Photography, 6

- Dennett*, on Fracastorius, 132
 — on Jupiter, 280
 — on Mars, 106
Denning, on a Ceti as a Radiant Point, 106
 — on Comet I., 1870, and the Perseids, 308
 — on the long duration of Meteor Showers, 30
Downing, on the Elements of the Asteroids, 17
 — on the Proper Motions of the Edinburgh Catalogue Stars, 129
Dunkin, The Solar Eclipse of 1851, 305
 — The Transit of Mercury, 142
- ECLIPSE OF THE MOON, Aug. 12, 1878, 209, 256
 — Sun, 1851, 305
 — July 29, 1878, 294
 Remarks on the Observations of the, 241
 Electricity and the Solar System, 355
Ennis, on Electricity and the Solar System, 255
Erck, Improvement in the Spectroscope, 33, 81
 — A Portable Equatorial Mounting, 76
 — on the Satellites of Mars, 20
 — A Substitution for the Declination Circle, 32, 102
 Equatorial Mounting, A Portable, 76
 η Geminorum, 104
- FIESOLE, THE NEW METEOROLOGICAL OBSERVATORY AT, 161
Flammarion, on the star Lalande 31266-7, 183
 Fracastorius, 132
Franks, on the Comets of Rigel, 184
 — on Stars Possibly Variable, 184
- Gardiner*, on the Satellites of Jupiter and Saturn, 105
Gill, Results of Observations of the Mars Comparison Stars, 289
- Globe, Discovery of an Arabic Celestial, 23
Gore, on β Corvi, 158
 — on Lalande 31266-7, 160
 — on Some Stars Possibly Variable, 159
Gribble, Canon, Death of, 283
Grubb, Improvements in the Spectroscope, 33
Guyon, on the Shadow of Titan, 17
- Hall*, The Opposition of Mars, 2
 Horizon, Artificial, 75, 129
Huggins, on the Heat of the Stars, 309
Hunt, on Uranus, 107
 Hyginus, The New Crater near, 229, 254, 269
- Innes*, on the Satellites of Jupiter, 128
- Jannsen's* Solar Photographs, 6
Johnson, on an Artificial Horizon, 129
 — on Bacon and the Telescope, 255
 — on the Comets of Rigel, 157, 183
 — σ^2 Piscium, 78
 Juno, Ephemeris of, 216
 Jupiter, 234, 255, 256, 280
 — The Atmosphere of, 19, 42, 53, 77, 102, 130
 — Ephemeris for Physical Observation of, for :
 April, 112 Aug., 214
 May, 136 Sept., 238
 June, 165 Oct., 262
 July, 188 Nov., 286
 — The Satellites of, 128
- Klein*, Changes on the Moon's surface, 265
 — on the New Crater near Hyginus, 269
Knort, on ω^1 Cancri, 132
Kohl, on η Geminorum, 104
- Langley*, The Transit of Mercury, 1878, 217
Lawrence, on Comets, 78
Lettsom, on the Change of Colour in a Ursæ Majoris, 79

- Leverrier*, The proposed Erection of a Statue to, 118
Lindsay, Lord, on the Names on the Lunar Map, 306
 — Spectroscope for Examining Nebulae, &c., 9
 — The Transit of Mercury, 147
Linné, 105
 Lunar Nomenclature, 52, 158, 306
- Macdonnell*, on the Transit of Mercury, 207
McCance, on Lunar Nomenclature, 159
 Madras, The Longitude of, 161
 Mars, 106
 — The Opposition of, 1877, 2
 — The Satellites of, 20, 23
Marth, Ephemeris of Jupiter, 112, 136, 165, 188, 214, 238, 262, 286
 — The Inner Satellites of Saturn, 188, 214, 238, 262, 287, 313
 — the Satellites of Uranus, 27, 58, 84, 113
 Mercury in Transit, The Luminous Spot on, 119, 139, et seq.
 — The Transit of, 1878, 136, 139 et seq., 207, 219
 Meteor, The, of March 15, 1878, 161
 Meteors, 75
 Miraculous Vision, 17
 Moon, The, and Jupiter, 18
 — Changes on the Surface of, 265
 — Names on the Map of, 52, 158, 306
 Motions, Proper, of the Edinburgh Catalogue Stars, 129
- NEBULA, A NEW, 157
Neison, The Atmosphere of Jupiter, 42, 53, 102
 — Hansen's Terms of Long Period in the Lunar Theory, 88
 — on the New Crater near Hyginus, 229
 — on Newcomb's corrections of Hansen's Value of the Secular Acceleration, 299
Noble, on Names on the Map of the Moon, 52
Noble, on Specular reflection from Venus, 78
- OBSERVATORY, THE FIESOLE METEOROLOGICAL, 161
 — Madras, 73
 — Royal, 167
- ω^1 Cancri, 77, 132
 Oxygen Lines in the Solar Spectrum, 301
- PALLAS, EPHEMERIS OF, 234
 Parallax of the Sun, The, 2
Penrose, The Solar Eclipse, July 29, 1878, 294
 — The Transit of Mercury, 143
- Planets, The, at Transit over the Meridian of Greenwich:—

Jan.,	26	July,	185
Feb.,	58	Aug.,	211
March,	84	Sept.,	235
April,	112	Oct.,	259
May,	136	Nov.,	286
June,	164	Dec.,	312

 — New Minor, 216
- Plummer*, The Errors of a Transit Instrument from a mass of Brickwork, 92
 Polaris, 107
Proctor, on the Atmosphere of Jupiter, 19, 77, 130
 — The Determination of the Axial Position of Mars, 89
 — The Intrinsic Brightness of Objects seen with the Telescope, 90
 — on the Moon and Jupiter, 18
 — on the Royal Astronomical Society, 51
 — on the Suspected New Crater near Hyginus, 254
 — The Transit of Mercury, 143
- Ranyard*, on Edison's Tasimeter, 309
 — Iron Particles in the Atmosphere, 299
 — The Solar Eclipse, July 29, 1878, 241
 — The Transit of Mercury, 146

Ranyard, The Variation in the Gaseous Part of the Corona, 281

REVIEWS:—

Address at the Inauguration of the Fiesole Observatory, 228

American Journal of Mathematics, 272

Annals of the Harvard College Observatory, 221, 304

Annaire de l'Observatoire Royal de Bruxelles, 154

Ball's Astronomy, 12

Barclay's Astronomical Observations, 206

Bazley's Stars in their Courses, 274

Browning's How to Work with the Spectroscope, 229

Clissold's Divine Order of the Universe, 127

Dawson's Theory of Gravity, &c., 229

Dun Echt Observatory Publications, 123

Ennis' Astronomical Papers, 203

— Electric Constitution of the Solar System, 207

— Our Sideral System, 74

Flammariion, Etudes et Lectures sur l'Astronomie, 15

Fleming's Uniform Non-local Time, 273

Gore's Southern Stellar Objects, 229

Instructions for Observing the Total Solar Eclipse, 1878, 278

Lagrange, de l'Origine, &c., des Mouvements Astronomiques, 277

Langley's New Method in Solar Spectrum Analysis, 14

— Transit Observations without Personal Error, 14

Lloyd's Miscellaneous Papers connected with Physical Science, 128

Lockyer's Star-gazing, 278

Lohrmann's Lunar Map, 197

Meteorological Observations made at the Adelaide Observatory, 16

Moore's Sun, &c., 253

Newcomb's Popular Astronomy, 270

Observations made at the Royal Observatory, Edinburgh, 226

REVIEWS continued:—

Proctor's Cycloid and other Curves, 97

— Myths and Marvels of Astronomy, 227

— Other Worlds than Ours, 98

— Star Atlas, 153

— Transits of Venus, 154

— Universe of Stars, 153

Report of the Harvard College Observatory, 221

— Stonyhurst College Observatory, 156

— Temple Observatory, 155

Results of the Greenwich Astronomical Observations, 152

Sebafarik's Visibility of the dark side of Venus, 75

Stewart and Tait's Unseen Universe, 98, 198

Symons' British Rainfall, 16, 253

Thornthwaite's Hints on Reflectors and Refractors, 253

Transactions of the Royal Irish Academy, 102

Rigel, The Comes of, 157, 183, 184

Royal Astronomical Society, The, 51

ROYAL ASTRONOMICAL SOCIETY'S MEETINGS:—

Dec. 14, 1877, 1

Jan. 11, 1878, 29

Feb. 8, " 61

March 8, " 87

April 12, " 115

May 10, " 139

June 14, " 168

Nov. 8, " 289

Rutherford, Photographs of the Solar Spectrum, 149

— Sun, 149

— The Transit of Mercury, 142

SATURN, EPHEMERIS OF THE INNER SATELLITES OF, 188, 214, 238, 262, 287, 313

Schuster, The Solar Eclipse of July 29, 1878, 295

Secchi, Memorial to, 166

Slack, on Jupiter, 234

— and the Lunar Eclipse, 256

- Spectrum, Photography of the Least Refrangible End of the Solar, 115
 Star, Lalande 31266-7, 160, 183
 — Σ 951, 104
 — σ^2 Piscium, 78
 — 53 Virginis, 130, 157
 Stars, Heat of the, 309
 — Possibly Variable, 159, 184
 Stone, The Telescopic Observations of the Transit of Venns, 95
 Sun, Curious Appearance of the, 161
 — Spots and the Weather, 191
 Taplin, on Comets, 131
 Tasimeter, Edison's, 309
 Tebbutt, on the Great Comet of 1861, 208
 Titan, The Shadow of, 17
 Tupman, The Measurements of the Transit of Venus Photographs, 170
 URANUS, 107
 — The Satellites of, 27, 58, 84, 113
 VENUS, THE DARK SIDE OF, 76
 — Specular Reflection from, 78
 — The Transit of, and Photoheliography, 54
 Vulcan, 251
 Walker, on 53 Virginis, 157
 Ward, on Σ 951, 104
 Waters, The Distribution in Space of the Southern Fixed Stars, 36
 Webb, on the Dark Side of Venus, 76
 Wilson, A Special Case of the Probable Result of a Number of Observations, 1
 Wolf, Sun-spots, 3

ENGRAVINGS.

- District surrounding the New Crater near Hyginus, 268
 Erek's Combined Position and Setting Circle, 103

Notices to Correspondents, &c., &c.

•

•

•

The Astronomical Register.

No. 181.

JANUARY.

1878.

ROYAL ASTRONOMICAL SOCIETY.

Session 1877—78.

The Second Meeting after the long vacation, Friday, December
the 14th, 1877.

William Huggins, Esq., D.C.L., LL.D., &c., *President*,
in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

The following gentlemen were elected Fellows of the Society

C. J. Lambert, Esq., 1, Crosby Square, E.C.

Arthur Schuster, Esq., Ph. D., of St. John's College,
Cambridge.

Mr. Glaisher said the number of presents received since the last meeting was fifty-six. Of these only one calls for special notice. It is a chart showing the lines of magnetic variation as observed in 1700, by Edmund Halley. It has been presented to the Society by Mr. J. E. H. Peyton, who also gave us the diagram showing the path of the shadow of the total solar eclipse of 1724, which is exhibited in the assistant-secretary's room. I am told there is a copy of the chart in the British Museum, but that there is none at Greenwich, and that it is nearly unique. It is entitled "A new and correct chart showing the variations of the compass in the western and southern oceans in 1700, issued at His Majesty's command, by Edmund Halley."

The thanks of the Society having been voted to Mr. Peyton and the other donors of presents.

Mr. Ranyard read a paper by Mr. Wilson, entitled *Note on a special case of the most probable result of a number of observations*. There is one law of weighting observations which lends

VOL. XVI.

itself so easily to calculation that it will readily be understood by non-mathematical readers. In order to find the most probable result of a series of observations, the arithmetical mean may be taken as a rough approximation. But if the arithmetical mean be taken there is no possibility of choice, the observations must be accepted wholly or rejected wholly. But supposing we agree that observations shall have less weight the more remote they are from the final result, in fact that the weight of an observation shall vary inversely to its distance from the final result. Let the observations be arranged in order of magnitude. If the number is odd the middle one must be taken, and if the number is even anywhere between the two middle observations may be taken. This will readily be made apparent by means of the following mechanical analogy:—A number of rings of various weights may be supposed to be arranged upon a rod without weight, so that a ring of weight x is at a distance $\frac{1}{x}$ from the centre of the rod. Each ring will thus have a moment 1 about the centre of the rod, and if the number of rings on each side of the centre of the rod be equal, the rod will balance about its centre; and if the number of rings be odd the middle ring being placed at the centre the rod will balance about the middle ring.

Mr. Ranyard read parts of a communication from Mr. Gill, in which he mentioned that he had returned from the Green Mountain, and resumed his observations the same evening. "During the last month the weather had been unfavourable, and if the opposition of Mars had occurred in October the result would have been a complete failure, and only one series of morning observations would have been possible." There had been a good many clear evenings during which he had been occupied with the triangulation of the Mars' stars of comparison. Three good nights' work would now finish that part of the work. He had also taken a series of measures of α Centuri (1) and (2), and the result showed that there was a small increase in the position angle, and that the distance between the two was increasing. Next month he states he will commence his observations of Melpomene.

Mr. Ranyard mentioned that an important paper had been sent to the Society by Mr. Maxwell Hall, describing a series of observations of the opposition of Mars. The resulting parallax which he obtains, is 8".789, which does not differ very greatly from the transit of Venus result. The observations were made it appears with an equatorial instrument, they are morning and evening observations like those of Mr. Gill. Mr. Maxwell Hall has very rapidly reduced the whole of his observations, and has sent a copy of his observations and reductions at length. The paper has been referred to

a member of the Council, as is usual in such cases, and it will most probably be printed in the *Memoirs*.

Mr. Neison: What is the aperture of Mr. Maxwell Hall's telescope?

Lord Lindsay: I think it is four inches.

Mr. Neison: Four inches, of course, is a very small aperture to make such measures with.

Lord Lindsay: I think the observations have not been made as measures but taken as transits; the difference of time between the transit of the star and of the limb of the planet has been noted, and the time is given to two places of a second of time!

Mr. Ranyard: I should like also to draw attention to a very important paper that will appear in the 43rd vol. of the *Memoirs*, which is now ready for Fellows to apply for. It is a paper by Dr. Wolf, of Zurich, originally presented to the Society in German, but at the request of the Council Dr. Wolf has re-written it in French. Dr. Wolf has, as is well known, spent a great part of his life hunting up all the sun spot observations he could find, whether in MSS. or in printed form. He has plotted down curves from the information he has obtained showing the relative frequency of sun spots from the date of the discovery of the telescope to the present time. He begins with the MSS. of the English astronomer, Harriott, which are preserved at Petworth, these date from 1610, two years after the year usually assigned for the discovery of the telescope. He then proceeded to discuss the printed observations of Galileo, Scheiner, Gassendi, Riccioli, Hevel, Picard, Cassini, Maraldi, Lalande, and he has worked up the manuscripts of Horrebow at Copenhagen, of Hagen at Poulhova, of Mallet at Geneva, of Pastorff in our own library, of Schwabe, and of a host of others. The labour involved must have been very great, but the result is quite worthy of the labour that has been bestowed. Dr. Wolf has thus been able to present to the astronomical world a continuous curve showing the relative number of sun spots from 1745 to the present time, that is, during more than twelve sun spot periods. And he has succeeded in determining with tolerable accuracy the years of maximum and minimum sun spot development from 1610 to the present time, i.e., the dates corresponding to twenty-four periods of maximum development, and twenty-four periods of minimum development. One of the most interesting facts which Dr. Wolf brings out is that the average divergence of the sun spot period from the mean period of 11.11 years amounts to as much as ± 2.03 years, and the time which elapses between two consecutive maxima is sometimes as long as fifteen or sixteen years, while at other times it only amounts to seven years; for

example—at the beginning of this century between the maxima of 1788·9 and 1804·2, there elapsed 16·1 years; while between the maxima of 1829·9 and 1837·2 only 7·3 years elapsed. His curve also shows that the second subsidiary period of maximum development, which is usually on the descending branch of the curve, is very irregularly developed. So that the general appearance of the sun spot curve differs greatly from period to period, usually, however, it rises more steeply than it descends.

Capt. Noble: It would be a curious subject of research to find out whether the Madras famines have occurred at periods of 16 or 11 years, because the public has lately been assured that they follow the sun-spot curves, and if it be a fact that 16 years have elapsed between two successive maxima at one time, and then only 7 years at another, it would follow that the famines ought to recur in the same erratic manner: at least that is what we have been taught in the *XIXth Century*—I mean the periodical, not the epoch. (Laughter.)

The thanks of the meeting were returned to the authors of the communications.

Mr. Boys was called upon to give an explanation of an electric clock which he had devised. His chief object had been to construct a clock which should be as free as possible from all atmospheric disturbances. The pendulum was therefore made to swing in an exhausted glass cylinder, and the impulse communicated to it by means of two small electro-magnets outside the glass case. These were alternately made to attract the pendulum by means of a current which could be kept of constant strength by an ingenious clockwork contrivance.

Mr. De La Rue enquired whether the clock was to be used as a driving machine, or whether it was only to be used as a regulator.

Mr. Boys said it was only intended to be a time-keeper. At the bottom of its arc the pendulum swung through a globule of mercury and so made contact and passed a current which relieved a detent. In order to make the time of contact with the mercury as nearly as possible of equal duration during every swing, it would be well to have an elongated globule of mercury, so that the pendulum should always pass through approximately the same thickness of mercury, to regulate the clock the electro-magnets could be moved nearer or farther from the pendulum. It would be seen that the pendulum had been treated in the most theoretical way possible. The impulse is transmitted at the middle of the swing, and only during a very short period of time. The pendulum was in a vacuum, and therefore would not be affected by atmospheric changes.

Mr. Christie said: I have looked over this plan of Mr. Boys'

clock, and he seems to me to have hit on some very ingenious methods of getting over difficulties in the construction of astronomical clocks. Of course what would strike any one at first is that it is rather complicated, and I cannot say how it would work in practice, but it seems to me the practical difficulties might be overcome, and the only point about which there could be any doubt would be in the contact at the bottom of the pendulum. Mr. Boys asserts that, as the contact takes place in vacuum, there would not be any tendency to oxidation, which we find a serious difficulty in ordinary clocks, for whenever you have a spark you have oxidation. The electrodes burn, as it were, and they want constant attention, and sometimes sticking takes place, which alters the rate of the clock. I do not know how far the vacuum would get over that difficulty, but probably it would be very much diminished. Mr. Boys made out that there would be a very small battery power required, that is a very important matter, but even with a single cell battery power, as in use with the Greenwich clock, there is a considerable burning at the electrodes, where the springs are pressed together, and the variation in the rate of the clock seems to depend on the amount of the sticking of the springs. In fact, as far as we can judge, the clock would be perfect if we could dispense with that contact. It struck me several times—thinking it over—that if you could have a clock which would not be required to do anything (laughter) except drive hands on a clock face—that is, when I speak of not doing anything, I mean not registering on a chronograph, which is the usual duty of an astronomical clock, it would be nearly perfect, but when you require it to make contact and register on a chronograph, although it may be done by means of a relay, as in this case, using a small battery power in the first instance, and afterwards another distinct battery, there would still be the defect from the action of the springs.

Lord Lindsay: I quite agree with Mr. Christie so far as regards this difficulty of the contact. I do not think it would be advisable to use mercury for the bottom contact, because it always oxidizes, whether in vacuum or in the open air. There is a change always where there is a spark, as I know from experience. A film grows over the mercury which would affect the current. I have tried a great number of electric clocks, and otherwise made use of electricity in the observatory, but have found that you cannot place any dependence on a clock in which the driving power, that is, the power which is entirely relied upon for moving the clock, is electrical. So long as there is a weight, and the *controlling* power only is electricity, the clock goes well, but when the driving power is obtained solely from a constant current, the

changes and fluctuations which are continually occurring from smaller causes make the clock practically worthless as a time-keeper.

Mr. Ranyard : Does Mr. Boys know of any experiments with a pendulum moving in a vacuum—whether there was any leakage ?

Mr. Boys : I do not know of any experiments in a sealed vacuum, but I do of experiments with a pendulum in a vacuum, where there was a stuffing-box. I believe there was considerable leakage. I refer to Mr. Carrington's clock, but if you had a vacuum without a stuffing-box it could be sealed all round.

Mr. Ranyard : Could it be sealed by fusion of the glass so that there would be no leakage ?

Mr. Boys : Yes ; not by fusion but with cement.

Mr. Ranyard : You think there would not be leakage through cement ?

Mr. Boys : Large vacuum tubes are kept for years, and I suppose such a clock case could be kept for years. The vacuum tubes are kept with metal caps at each end, not sealed down by fusion, but with cement.

The President : I will ask you to return thanks to Mr. Boys for his explanation of his ingenious clock.

The President : In the list of presents alluded to to-night an important one was omitted. It is now placed upon the table, and consists of a large positive of the sun taken by M. Janssen, of Paris. I think Mr. De La Rue will be good enough to give us a few words explaining the method in which it was taken and the important details which are visible upon the plate. I will ask you to defer your thanks to M. Janssen for this present until you have heard Mr. De La Rue's explanation, because then, I think, you will be able to appreciate more fully the value of the gift.

Mr. De La Rue : Mr. President, I am very happy to respond to your invitation to make a few remarks with respect to this very beautiful example of astronomical photography. The photograph which is now on the table is twelve inches in diameter and is such are daily taken at the observatory at Meudon, within about an hour-and-a-quarter's drive from Paris. The instrument with which it was taken is not mounted equatorially, this mounting though convenient is not absolutely necessary, because we all know that a sun-picture is taken in a very small fraction of a second of time. By placing the telescope a little in advance of the sun it is possible to obtain a perfect photograph as soon as the sun has attained a position coincident with the axis of the instrument. I am very proud of this example of astronomical photography, inasmuch as the instrument with which it was taken

is made on the lines of the Kew photoheliograph, which I had the honour to design at the request of the Royal Society; that is to say, it is not taken in the principal focus of the telescope, but the image is enlarged by means of a secondary magnifier. The only difference, and that is a very important one, between this instrument and the original Kew photoheliograph is this, that both the object-glass and the secondary magnifier are corrected actinically without reference to the visible rays, in the same manner as Mr. Rutherford's photographic telescope, with which he produced his very magnificent photographs of the moon. In my opinion this picture, one of a series of magnificent photographs, is the finest example of solar photography I have ever seen. The whole surface is covered with minute granular markings. I may call to the recollection of the Society that in 1861 I photographed the sun on a scale of four feet to the sun's diameter, and I think there are some details to be seen on those photographs which correspond with the details on M. Janssen's; but, for want of a perfect secondary magnifier in 1861, their sharpness is not so great. But there was one phenomenon I noticed when I was at Meudon, which has struck me particularly as being entirely new, and it is a phenomenon which is fraught with importance to the future development of our knowledge of solar physics. If you will have a little patience with me I will draw on a large scale the granular markings which pervade the whole of the solar surface. Besides, the gradual shading towards the periphery of the sun, and besides the faculæ and sun spots and other well known solar phenomena, there are spaces which show the phenomenon I was talking about. Such are distributed so irregularly over the surface of the sun forming groups of granulation, that one is inclined at first to doubt whether the photograph is perfect, for it gives the impression that there are parts of the plate where the focus is not so sharp as in others. On the day that I was at Meudon, M. Janssen, who expected me, had prepared his apparatus to take some photographs in my presence, but the clouds prevented him from so doing. However, two had been secured just before my arrival, and on looking at one of them I was impressed very strongly by what you must permit me to call in homely and colloquial phrase a smudginess. I saw this, but I did not like to say, "Why, your plate has been badly cleaned," but I said "Will you permit me to look at the second photograph?" I looked at it, and precisely those parts of the disc which appeared to be smudged on the one plate were smudged on the other—in fact the photograph had rendered evident vortex movements of which we can form no conception whatever by referring them to terrestrial

storms and cyclones. The photosphere had been hurled up in vortices on various parts of the sun and over a large extent of it. One saw at once that in these might lie the origin of the luminous protuberances with which we are now so familiar. M. Janssen and myself in talking over it could not help simultaneously saying, what I had often said before, that the recurrence periodically of solar spots cannot be considered in any way the most important of solar phenomena. There are changes which take place from day to day and hour to hour, in some cases I might almost say minutes, which completely change the aspect of various parts of the sun, showing such an amount of activity that it is extremely necessary to study it in order to enable oneself to render an account of solar phenomena. Hence I return to an old story of mine—the importance of observing the sun in, what has been very much decried, a physical observatory, devoted specially to the purpose, and furnished with instruments not only for taking pictures that would give us the position of sun spots, but pictures on such a scale as would enable us to study minutely and watch the changes which are continually taking place on the sun's surface.

The President: I will ask you to return thanks to Mr. De La Rue for his very lucid explanation, and to M. Janssen for his valuable gift to the Society. I had the pleasure of meeting M. Janssen at the Scientific Congress, at Havre, a few months since, and had the opportunity of examining very carefully in detail these markings which Mr. De La Rue has so well depicted on the board.

Mr. De La Rue: I ought to add two or three words. These beautiful photographs are not taken in any superbly-built observatory, but in a sort of encampment in the park, contiguous to the ruins of the palace of Meudon, in a temporary, tent-like building. In such an edifice it is that this splendid work was done.

The President (to Mr. De La Rue): You did not state the diameter of the object-glass.

Mr. De La Rue: The focal length I cannot remember, but the diameter of the object-glass is about $5\frac{1}{2}$ -inches; and I should think the length of the telescope, speaking from recollection, would be about 7 feet 6 inches, besides the part beyond the secondary magnifier.

Captain Abney: I think the total length would be 12 feet altogether. I had an opportunity of seeing it when I was at Meudon in April last. M. Janssen showed me some of these magnificent photographs, and I pointed out to him what I called the fuzziness of the image, but he did not then seem to pay that

particular attention to it which he has since done ; he seemed to have thought they were due to atmospheric effects rather than solar tornadoes.

Captain Noble : There is a good deal in this positive which is very remarkable, but I cannot quite make up my mind that some of the apparent structure is not due to the texture of the collodion. It may or it may not be, but it is very curious.

Mr. De La Rue : Captain Noble can hardly have followed me completely. There were some very visible real and apparent defects, but by comparing the two photographs very critically, as I am wont to do, the smudged structure was repeated on both photographs, and shown not to arise from defects, but the real defects were not repeated.

Mr. Christie : In the photographs we have taken at Greenwich we have had the same sort of thing, a sort of structure that might pass for defects in the collodion, and we have found that they were repeated, but we never got photographs at all comparable to M. Janssen's. Ours were, of course, on a much smaller scale, but still this structure of the sun is distinctly shown ; you get it day after day. I may mention that, in the case of a spot in April last that broke out on the sun, there was a disturbance of the photosphere preceding the outburst which was very marked.

Mr. Neison : Can Mr. De La Rue tell us what interval elapsed between the two photographs taken at Meudon ?

Mr. De La Rue : I should think about a quarter or half-an-hour, not longer. They were taken in rapid succession, for the weather was very uncertain.

Mr. Lynn read a note by the Rev. F. Howlett *On a remarkable solar spot seen on October 31 and November 1*, and exhibited some careful drawings made at East Tisted, near Alton, taken by projection with a power of 120, and screen 5 feet 2 inches from the eye-piece. Mr. Howlett called particular attention in his note to a very distinct haze of a kind of bistre-brown colour, extending some 10" or 12" upon the penumbra beyond the margin of the umbra on October 31st, as well also as a considerably-sized nucleus of above 12" diameter within the umbra. Both these phenomena were scarcely to be distinguished on November 1st.

Lord Lindsay : The President has been good enough to ask me to give a description of the observations I have made with the small spectroscopic arrangement which I described last month. The instrument which we have been using has been put together at Dunecht, and is excessively rough and unsteady. We have been using it upon the 15-inch refractor, with a power of 930 to condense the light upon the slit. We have not at present

made use of a cylindrical lens. I can fully bear out what I advanced at the last meeting, namely, that the spectroscopic lines or images derived from the light of faint nebulae appear considerably more distinct than the nebulae themselves do when they are viewed with the telescope in the ordinary manner. The great nebula of Orion gives a brilliant continuous spectrum from the stars contained in it. There are also bright lines lying on the continuous spectrum, which can be seen with such distinctness that the colour of the 4th line, the G line, of the spectrum, which I think is rather a rare line to see at any time, is decidedly marked.

Mr. Christie said he did not like making remarks upon a form of spectroscope which he had not seen and examined. He thought that Lord Lindsay might have obtained some advantage in point of brightness over the ordinary form of spectroscope, in which for use on faint objects the power was too high, so that a very narrow pencil only would reach the eye. Lord Lindsay's plan is to substitute for the image of the object a very small image of the object-glass formed by a condensing lens. But it would be evident that no advantage could be gained by making use of a lens to condense the image on the slit, for it would necessitate the use of a shorter collimator, with which a narrower slit would be required to give the same purity of spectrum. Mr. Christie then endeavoured to show, by means of a figure on the black board, that the short collimator and telescope would magnify the image in a way that would make up for the extra condensation of light upon the slit-plate. He did not wish to depreciate Lord Lindsay's experiment. Possibly some advantage had been gained, or appeared to have been gained, by means of the relatively broader slit which would thus be made use of; but what he wished to point out was, that there must be a loss of light with a condensing lens, and that, theoretically, you could with the ordinary spectroscope obtain a similar result by using a slit of sufficient width and low magnifying power.

Mr. Ranyard said that he thought Mr. Christie had not rightly understood the arrangement proposed. He had himself mistaken Lord Lindsay's meaning at the last meeting, and it was not until it had been explained to him that the slit was not to be placed in the primary focus, but behind the eye-piece, where the parallel rays crossed in what Lord Lindsay had called the image of the object-glass, that he saw that the spectroscope was really an integrating spectroscope, which picked up rays coming from various parts of a small area of the heavens. With a slit placed in the primary focus, it was evident, as Mr. Christie had pointed out, that the spectral images could never be as bright as the elementary

area of the nebula from which they were derived. This might be seen still more clearly if we considered the analysing tele-spectroscope as corresponding to two telescopes placed one behind the other, the front telescope having the collimating lens for eye-piece, then the shorter the collimator the greater would be the power of the front telescope, and the observing telescope remaining unaltered, the greater would be the magnifying power of the whole combination. In order that the whole pencil of rays from the light-collecting instrument may enter the pupil of the eye, it is necessary to make use of a combination of lenses that will give a total power to the tele-spectroscope corresponding to the ratio between the diameter of the light-collecting instrument and the diameter of the pupil of the eye. This fixes the total power of the combination which must be used for any particular aperture of light-collecting instrument, and if the image in the primary focus is reduced it will afterwards have to be magnified again by means of the collimating lens and viewing telescope, so that no advantage will be gained with regard to the breadth of slit that can be used. For the shorter the collimator the narrower must be the slit in order to obtain any required degree of purity of spectrum. It will thus be seen that with an analysing spectroscope it does not matter whether the focal length of the light-collecting instrument is long or short, and placing lenses in front of the primary focus will only absorb light and do no good. But the instrument proposed by Lord Lindsay was an integrating spectroscope,* and it was well known that with an ordinary integrating spectroscope the bright lines of an aurora could be seen, though the auroral light was so faint that it could not be detected without the instrument. With a telescope in front of an integrating spectroscope, rays would be picked up and integrated from a smaller area of the heavens. Thus if the integrating spectroscope, used without a telescope, would pick up and integrate rays from an area in the heavens 10° in diameter, then with a telescope with a power of 600, placed in front of the spectroscope rays from an area of only a minute in

* [Note by Mr. Ranyard.] If we suppose the surface from which light is integrated to be of uniform brightness, it is evident that the chromatic images of the slit, whether they are magnified by a collimator and telescope or not, cannot be as bright as an element of the surface from which light is integrated subtending an equal angular area. With a telescope in front of the slit there will, of course, be some loss of light by absorption and reflection from the surfaces of the lenses, and the area from which light is integrated will be reduced in the same proportion as the intensity of the light upon the slit is increased. Probably the reason why the integrating form is more effective, is because the collimator is always filled with a pencil of light, and is, as it were, self-adjusting, for with a larger collimator a larger surface is integrated from.

diameter would be integrated. There would, of course, be a certain loss of light, due to absorption and reflection from the surfaces of the lenses of the telescope, but putting this out of consideration the effect in examining a nebula of one minute in diameter would be the same as if the light from a nebula 10° in diameter, and of equal brightness, area for area, were being examined with an integrating spectroscope without a telescope in front of the slit. It would be easy to understand how the lines of a nebula area integrated in this manner might appear brighter than the nebula itself, for the nebula as seen with the eyepiece is partly drowned by the light dispersed by our atmosphere. This would be spread out into a continuous spectrum by means of the spectroscope, and the two or three chromatic images of the nebula might no doubt, as in the case of a faint aurora, be seen standing out with greater contrast upon the background of continuous spectrum than the nebula itself upon the illuminated sky.

Mr. Christie said he must apologize to the meeting for speaking again on this subject. Mr. Ranyard has put it in rather a new light, but at the same time the same law would hold, and the brightness of the nebula could not be increased whether the slit were in the primary focus or behind the eyepiece. What Mr. Ranyard had said with regard to the illumination of the atmosphere would equally be true with an analyzing spectroscope.

The following papers were also laid upon the table and partly read :

S. W. Burnham : *Ninth catalogue of new double stars, discovered with the 6-inch refractor.*

J. Tebbutt : *On Jupiter's third satellite.*

H. Pratt : *Notes on Mars, 1877.*

Major-General Addison : *Note upon the determination of the longitude of Kurrachee and Madras.*

REVIEWS.

Astronomy. By Professor R. S. Ball, LL.D., F.R.S., Royal Astronomer of Ireland. London : Longmans, Green & Co. 1877.

This little book is one, we believe the first published, of the London Science Class-Book Series, edited by Messrs. Carey, Foster, and Magnus, to be brought out, if possible, at the uniform price of 1s. 6d ; it being intended that each of the chief branches of science shall be represented by one or more volumes of the series. Of course such a wide subject as astronomy cannot be very exhaustively treated of in a manual of this size, but we were agreeably surprised, on looking into the book, to find such a quantity of information comprised in it, in which respect it will compare favourably with other text-books of the same pretensions.

The author only assumes a knowledge in his reader of as much geometry as is contained in the first three books of Euclid, and merely suggests that it will be greatly to his advantage to have some acquaintance

with trigonometry; notwithstanding the small amount of mathematical knowledge supposed to be possessed by the reader, such subjects as the figure of the earth, instrumental errors of the transit instrument, planetary orbits, the law of gravitation, parallax, including the transit of Venus, the precession of the equinoxes, and the aberration of light are treated of, and intelligibly enough explained, considering the great impediments to lucid explanation such restrictions are to an author, and generally make a non-mathematical book on astronomy much harder reading than any amount of mathematics.

In treating of the diurnal motion, Dr. Ball puts his reader on the right road for understanding the subject, by recommending him to watch the stars for himself, particularising, for the learner's benefit, the Pole Star and Great Bear; though, as he suggests that the student should continue his observations through the whole of the night and the following day, we fear that an extraordinary amount of zeal would be required for the due following out of the suggestion, and that it is hardly wise to begin by putting such a strain on the system, which may well damp the ardour of the most enthusiastic lover of science. Dr. Ball, as mentioned above, explains the instrumental errors of the transit instrument very clearly, and also the method of determining the zenith point correction of the meridian circle by the Nadir observation, we think he might advantageously have also given the method of determining it by reflexion observations of stars, as the more generally trustworthy of the two, and not more intricate to explain; but as these subjects are generally omitted altogether in elementary text-books, we are glad to find them referred to at all; the consequence of such omission being that when the student comes to use an instrument for himself, he is surprised and often disgusted to find that such a large proportion of his observations serve merely for the purpose of determining his instrumental errors.

In his chapter on the apparent motion of the sun, Dr. Ball again taxes rather severely the zeal and patience of his reader, as he says that it will be "only necessary to look at the heavens at a fixed hour on a series of nights throughout the year," to discern that the sun moves amongst the stars; we fear that in these times life is rather too short to allow us to spend a whole year in learning even such an important fact as the apparent motion of the sun.

There is another statement with reference to the sun to which we must take exception; in describing the method of determining the clock-error by observation of the sun, Dr. Ball adds "in our climate culminations of the sun cannot be very frequently observed." Now, this is not the objection to the use of the sun as a "clock-star" (indeed a reference to a volume of Greenwich observations shows that the sun can be observed on the meridian on about half the days of the year) but the objection would still hold good even if we were certain of observing him at every culmination; the fact is that the motion is not even yet sufficiently well known to enable us to predict his place with the accuracy required by modern science, and, in addition to this, it is impossible to observe the transit of the sun with the same accuracy as that of a star.

The chapters on Kepler's Laws, parallax, precession and aberration, please us much, and show that the author is quite at home in explaining abstruse phenomena to a youthful audience so that they may grasp the general principles, and not have their minds confused with tedious details. This excellent manual closes with an interesting account, as far as it goes, of double stars, nebulae, and spectrum analysis, and is well

calculated to introduce the student to more advanced works on these subjects; to show that it is well up to date we need only state that the lately discovered satellites of Mars are mentioned in the text.

We cordially welcome this little volume, coming as it does from the pen of one of the Professors of the "Silent Sister," and we hope that some of the author's colleagues may be induced to follow so excellent an example, so that henceforth we may more frequently see their names on the title pages of scientific works.

On the Possibility of Transit Observation without personal error. By S. P. Langley. (From the *American Journal of Science and Arts*, Vol. XIV., July, 1877.)

In this method the dark field is illuminated by flashes of light from the electric spark, regulated at pleasure by a small clock with a conical pendulum and two contact wheels. After describing the apparatus Mr. Langley says: "Before the transit of any star the observer adjusts the conical pendulum beside him (this is the work of but a few seconds), and then seats himself at the instrument holding the cords in one hand, like the 'reins' of an equatorial. If a flash occurs just as a star is crossing the first wire (which is most unlikely) he has nothing to do, except possibly to notice which was the middle wire, for each records itself on the chronograph without any intervention of his. But if the star be, for instance, two-thirds of the way between the first and second wire at the first flash, he will draw one of the cords, accelerating the flash, and thus causing the star to appear nearly coincident with the second wire when the next spark comes, and repeat the adjustment by the light of subsequent flashes, till the bisection is perfect. Three or four trials are in practice found to yield a bisection which will satisfy a fastidious eye, and when a satisfactory one has been once made, the effect is automatically repeated. . . . Under the general conception, then, of the possibility of diminishing to any limit personal error, by employing brief views of the star or wire and utilizing the phenomena of persistence of vision, the particularly described device assumes to dispense with the observer's record upon the chronograph altogether, and to substitute a purely automatic one, giving the same virtual result as though the image of the star were a tangible object, itself making electric contact with each wire. The share of personality in any observation is relegated to the prior act of bisecting a star, virtually motionless with relation to the bisecting wire, so that if (as seems to be the case) this act is independent of quickness or slowness of perception, of the time of cognition, or of the speed of nerve transmission; personality, in the technical sense, appears not to intervene at all."

Mr. Langley says this method has succeeded well so far as yet tried at the Allegheny Observatory. It is especially intended for longitude work. It seems to us very promising.

A Proposed New Method in Solar Spectrum Analysis. By S. P. Langley. (From the *American Journal of Science and Arts*, Vol. XIV., August, 1877.)

"No observation of modern physical astronomy is more striking in its conception, than that which attempts to determine the motion of a celestial body by the altered wave-length of its light, and none has attracted more general attention. It is popularly understood, I think, that the proper motion of certain stars in the line of sight has been thus completely demonstrated, but those particularly engaged in such studies know how

far astronomers have till very lately been from the certainty attributed to them. . . . The theory of the proposed method is very simple. Let two spectra be formed side by side, the one of light from one edge of the sun, the other of light from a point 180° distant. The instrument being in adjustment, if these points be in the neighbourhood of the solar poles, which are relatively at rest, all the lines will be continuous in both spectra. But if the instrument is rotated till the light comes from points in the eastern and western sides of the sun which are in relative motion, not only will the solar lines be discontinuous, in the two spectra, as though the one receiving light from the advancing or eastern side had been slid past its neighbour toward the violet, but any maladjustments of the instrument, which simulate this effect, can be with certainty detected by a means to be shortly described. The actual quantity by which we may anticipate from theory that the spectra are displaced is, as has been observed, extremely small, and to produce the desired result we need not only very great dispersion in the spectroscopic, but very delicate workmanship in the cutting and mounting of the prisms which are to bring the light to the slit, and in general it will be evident that more than common skill is to be desired of the instrument maker.

"I wish to acknowledge the obligations I am under in these respects to Mr. F. Walker, of Philadelphia, the optician, and to Mr. William Grunow, the maker, of New York. Not to dwell on those difficulties of detail which always come between the conception and its embodiment in practice, I may say that more than six months have passed in experiment and modification, till the instrument has attained its present form, in which I have been finally able within the past few days to see my anticipations justified. That it has been possible to me to undertake this research at all at present is due to Mr. Rutherford, who has given me choice specimens of his gratings, which are so generally known and valued, that it is unnecessary that I should describe them." A description follows of the instrument and its use, and towards the end the author observes, "I ought to add that the proposed method is considerably limited, not only by the need of very powerful instruments (for these can be supplied), but by the need of very good observing weather, which unfortunately we have not at command when we want it."

We anticipate that valuable work will be accomplished by Mr. Langley with his new instrument.

Etudes et Lectures sur l'Astronomie. Tome Septième. Par Camille Flammarion. Paris, Gauthier-Villars, 1877. [David Nutt, London, 2s. 6d.]

The author had intended to devote this volume to the exposition of the calculations of the parallel of the sun, and the results of the late transit of Venus, but as the astronomers of different nations have not yet published the results of their labours, the completion of his intention is reserved for the next volume, and the present is devoted to planetary and cometary astronomy. In it we have many interesting details about Mars; Jupiter, and his satellites; the satellites of Uranus; Spectrum Analysis of the Planets; the occultation of Venus, October 14, 1874; Eclipses of the Sun and Moon, 1874, 1875; the minor planets to 164 inclusive; the earth as seen from the different planets; and the most recent comets observed, 1872—1875. We have been much interested in this little volume, which has many illustrations, and cordially recommend it to our readers. We need hardly repeat our former commendations of M. Flammarion's valuable series of "Studies and Lectures," of which this forms the seventh volume.

Meteorological Observations made at the Adelaide Observatory, under the direction of Charles Todd, C.M.G., F.R.A.S., Observer, Postmaster-General and Superintendent of Telegraphs. Published by authority of the Government of South Australia.

These observations, in separate monthly numbers, extend from April to September, 1876. The hours of observation are 9 a.m., noon, 3 p.m., 6 p.m., 9 p.m. An introduction prefixed to each number gives an account of the instruments, their location, &c. Two pages of very complete and clearly arranged observations comprise 1. Barometer and the various thermometer readings 2. Ozone, cloud, wind (upper currents also are noted), rain and evaporation. Another page is occupied with abstract of results, including humidity, solar and terrestrial radiation, temperatures of soil and sea, and evaporation. A table follows of the number of times the wind has blown from each point of the compass, and another of the rainfall in the colony generally observed at 78 stations. The last page gives the meteorological curves for barometer, wind, force of ditto, temperature (max. mean time), relative humidity, ozone, cloud, rain and evaporation. General remarks accompany these observations, and meteorologists will be thankful to the observer for this monthly series, and to the Government of S. Australia for its publication.

British Rainfall, 1876. On the distribution of rain over the British Isles, during the year 1876, as observed at about 2,000 stations in Great Britain and Ireland, with maps, and illustrations. Compiled by G. J. Symons, Secretary of the Meteorological Society; Membre de la Société Météorologique de France, &c., &c., &c. London: Edward Stanford, Charing Cross, S.W.; Simpkin, Marshall & Co., Stationers' Hall Court. 1877. (pp. 223. Price 5s.)

"Very few people," says Mr. Symons, "have the remotest idea of what is implied by the familiar word 'million'; if, therefore, I said that the preparation of this work involved the examination of more than a million figures, few persons would realise what the statement implied. [See remarks about this in Sir Edmund Beckett's *Astronomy without Mathematics*, pp. 76, 77. 5th ed.] It will, perhaps, give a better idea of the labour expended if I state that before one figure was printed, upwards of 20,000 columns of figures have been checked, and that every error detected in any one of them has been the subject of communication to the person who made the mistake." "Statistical work is good for nothing if it is not accurate, and though it is extremely difficult to ensure accuracy in such a work as this, I hold it to be my duty to spare neither money, time, nor thought in attaining as nearly perfect accuracy as is humanly possible." * * * "There being, at present, considerable misapprehension as to the amount of Government aid granted to meteorology, it may be well to state that the expenses of the system of which the following is the annual report, are entirely borne by the gentlemen named on pages 13 to 15, and by the profits of the sale of this work, while the observations are made gratuitously by nearly two thousand observers."

Rain has so much to do with agriculture, water supply, health, many of the circumstances of life, various branches of science, and sometimes with life and property, that such an elaborate and accurate work as Mr. Symons brings out annually should be interesting to farmers, local boards, medical men, engineers, private gentlemen in towns and the country, besides official and amateur meteorologists, and many others might (and no doubt in many cases they do) find profit from it. Who is

there in fact whom rain does not concern more or less? This work well deserves a more extensive circulation. It should be found in all public libraries, and in as many private ones as possible. The excellent economy of Mr. Symons makes the utmost of still inadequate funds: a subject on which in future years we trust he will have no occasion to remark. To himself, his assistants, and supporters our thanks are due for another volume of permanent value and considerable interest.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

"MIRACULOUS VISION."

"*The days of our youth are the days of our glory.*"—BYRON.

Sir,—The acute eye of the Baron von Erthton's nephew would not be so exceptional if astronomers would call, as I do for the moment, upon young people gifted with extraordinary eyesight, teaching them merely the way of managing the optical part of the instrument, and instruct them how to use their eyes to the best advantage. Louis Figuier says that the brain in youth is as soft wax, receiving easily the impression made on it. The retina is equally gifted with a marvellous sensibility which fades soon by work, fatigue, and excess of every kind. I can give a new proof of what I write. On the 2nd of this month, under the best circumstances, I mean, the sun being almost at his greatest distance from the pole, and the sky exceedingly transparent, a boy, not yet seventeen years old, saw, in the afternoon at one o'clock, the companion of the Pole Star, with my 6½-inch refracting equatorial. He did not know that the star was double, and said to me—"I see a very minute one close to it." The position he indicated was that of the double's place.

As regards the two new companions, two more confirmations of their existence may be found in the *English Mechanic*, 28th July, 1876, and p. 215—1877.

I am, sir, your obedient servant,

Place de Meir 19, Anvers:

AD. DE BOË.

Dec. 12, 1877.

ELEMENTS OF ASTEROIDS.

Perhaps it may be of use to some of your readers to learn that a Table of Elements of the Asteroids, (1)—(172), has been published in the *Smithsonian Report* for 1876, pp. 361—363.

The Table appears in a paper by Prof. Daniel Kirkwood, of Indiana, on "The Asteroids."

A. W. DOWNING.

SHADOW OF TITAN.

Last evening, December 9th, I obtained a good view of the transit of Titan's shadow over the disc of Saturn. The weather was so cloudy that

the planet was completely invisible for some time after my going into the observatory, and the prospect seemed dismal enough, but a momentary sight of Saturn revealed a spot on the southern portion of his disc. As time wore on the sky cleared and the shadow was plainly seen to be nearer the planet's equator than its pole, and somewhat eastward of its meridian. This was at 23h. 35m. sidereal time. Clouds prevented sustained observation, but at 23h. 45m. I judged it to be on the meridian, and at 23h. 59m. it was evidently westward of the same. At oh. 50m. it might have been two-thirds across the disc, and at 1h. 40m., when I went again to the observatory hoping to see Titan off the face of the planet, there was no trace of the satellite, but the shadow seemed within two or three diameters of the western limb. These observations are not very exact, but the phenomenon was rather a trying one for a telescope the size of mine, which is a refractor by Wray, of only 3½-inch aperture, using powers of 220 and 320. At the same time I noticed four luminous points in Saturn's immediate neighbourhood, which I should be greatly obliged if Mr. Martz would identify with any of the satellites. Two preceded the planet about six and seven semi-diameters, one of them very faint. One followed at a distance of five semi-diameters, and another was about one-and-a-half semi-diameters south of the preceding ansa, and rather within its extremity. This from its distance above the plane of the others, if not Japetus, must, I presume, have been a star. A transit of Titan will take place again on December 25th at 5h. 45m., and perhaps on January 10th at 5h., after which it appears that the phenomenon will not recur for fourteen years.

G. GUYON.

Ventnor, Isle of Wight:
December 10, 1877.

THE MOON AND JUPITER.

I cannot follow Mr. Neison in his somewhat unusual course of discussing in one publication (not read by the persons chiefly concerned) a matter raised in another. This, therefore, must be my only communication here on the subject of his letter, and must be very brief.

My original remarks about Mr. Neison in the *English Mechanic* were not at all strong, and assumed his entire innocence of plagiarism. What was strongly worded in later letters (Mr. Neison remaining wholly silent till W. G. P. asked what he had to say) related to a correspondent, "N," who had asked both W. G. P. and W. C. E. for information when the two inequalities were under discussion by them. That was eight or nine months before Mr. Neison's communication on the same subject to the Astronomical Society. If Mr. Neison is not "N" he is not affected by those remarks, if he is they are just.

In his letter to you he admits all that I have ever regarded as essential, or have so described in the *English Mechanic*.

His reason for not mentioning W. G. P.'s name is no doubt sufficient, but it does not appear to me (nor does W. G. P. regard it) as explaining Mr. Neison's silence respecting W. G. P.'s communication.

Mr. Neison's "good-nature," in not wishing to say more about myself, is perhaps not really so "foolish" as he says. He has at least tried the effect of an opposite course, his foolish good-nature not being so well developed a month or two ago, as to prevent his writing to the editor of the *English Mechanic* a letter so "unusually personal" that it was returned to Mr. Neison for the excision of certain passages. Nor did his "foolish good-nature" prevent him from forwarding that production to myself, with

the personal passages underlined. The opening sentence, however, which was not underlined, was altogether sufficient for me, and I am unable to say what the underlined passages really were.

There was nothing in my original communication which would not have been entirely met by a frank (though tardy) acknowledgment on Mr. Neison's part, of his indebtedness to W. G. P. (*Sera nunquam*, &c.) He has preferred contesting details, while obliged to admit essentials. It is not for me to decide whether in so doing he has shown wisdom or the reverse. But certainly I do not consider his "good-nature" was foolish; on the contrary, I think it was singularly well advised.

Nov. 9, 1877.

RICHARD A. PROCTOR.

[After printing Mr. Neison's letter we feel it necessary to insert Mr. Proctor's reply, but in so doing we do not wish to be considered as indorsing Mr. Proctor's opinion with regard to the propriety or impropriety of the course pursued by Mr. Neison, when in making a communication to the Astronomical Society, with reference to the value of certain terms in the Lunar Theory, he omitted to refer to the fact that a writer in the *English Mechanic*, signing himself W. G. P., had drawn attention to the existence of the terms, but had stated that the term of long period was insensible.

Mr. Proctor has with much knight-errantry taken up the cause of the writer in the *English Mechanic*, but it seems to us that until Mr. Neison has published the steps by which he obtained his evaluation of the terms the matter remains *sub judice*.—Ed.]

THE ATMOSPHERE OF JUPITER.

Sir,—Mr. Neison strangely misapprehends the formulæ by means of which he proves to his own satisfaction that the depth of Jupiter's atmosphere cannot exceed a certain number of miles. If he had noted that Laplace and others who, like Ivory, have dealt with the subject of atmospheric refraction, have not been prevented from considering the possibility of far deeper atmospheres or even of the former entire gaseity of planets, he would have been saved from the strange error into which he has fallen. It is hardly necessary to explain to most of your readers, I suppose, that the empirical formulæ relating to atmospheric temperature, pressure, &c., employed by Ivory in dealing with atmospheric refraction cannot safely be used outside the limits within which Ivory actually used them. They are altogether inapplicable to the subject of Mr. Neison's present inquiry. Nor are they applicable to that with which he dealt some months ago, the subject of refraction by the moon's atmosphere. For though Ivory could safely include the refractive effects of the upper strata of our own atmosphere, knowing as he did that they produce but a very small proportion of the entire refraction, it is incorrect to apply the formulæ to a case where the whole refraction is produced by an atmosphere much rarer than our own.

Mr. Neison's mistake may be thus illustrated. Suppose a surveyor had shown that the curvature of a certain mountain slope (in a vertical plane) between two stations, might within certain limits of error be represented by a certain arc of an inverted parabola. Then anyone would be safe in estimating the elevation of any point between these stations, from this parabolic curve. Nor, if the curvature of the mountain were obviously continuous would any serious error arise from dealing with points a short distance above or below these stations, as lying on the extension of the same arc. But it would be unsafe to assume that on the

one hand the lowest part of the valley occupied the precise position of the parabola's vertex, which on either side the surface extended to an infinite height like the branches of the inverted parabola. Mr. Neison has managed to make a corresponding error of each kind—one in applying an atmosphere of great tenuity, the other in applying to an atmosphere of great density, the formulæ which Ivory found fairly accordant with the refraction motion of the terrestrial atmosphere.

Nov. 9, 1877.

RICHARD A. PROCTOR.

SATELLITE OF MARS.

Sir,—Since my last communication to you, I have succeeded in making some further observations both of the place of the satellite, also of his apparent brightness, and consequent size. Thinking these may interest some of your readers, I give them to you in detail.

Sept. 27, 23^h 0 G.S.T.—The satellite is again visible. Mean of two measures of position 69°. Distance perhaps three diameters. It is exceedingly faint, I can scarcely see it. The moon is up, but a long way off.

Oct. 2, 24^h 0 G.S.T.—I think, but only think, I see the satellite at about 70°; however, on referring to the ephemeris, I find that such was its place at that time. This was the last time I saw the satellite, though I looked for it, under favourable circumstances, on the 4th, 8th, and 11th Oct.

At transit, on the 27th Sept., when the satellite had become all but invisible, the log. distance of the planet was 9.6250, and the log. rad. vector was 0.1434, so that the brilliancy of the planet at this time was but 0.82 of his brilliancy on the 5th Sept., 1877, the day of opposition.

Of all the oppositions since 1849 the only one at which the brilliancy exceeded 0.82, was that of 17 July, 1860, when the brilliancy was 0.93, but the twilight at that season of the year would probably have extinguished the satellite.

Similar considerations will probably suffice to show how easily the satellite may have escaped detection in former years.

It does not appear probable that the satellite varied much in brightness, for it was as well seen in the preceding as in the following half of its orbit; in the former from 238° to 270°, in the latter from 55° to 78°.

As to the size of this satellite, seeing that it does not present any sensible disc (indeed the actual disc cannot be a tenth of a second) it only remains for us to guess at the size from the brightness, on the assumption that the surface of the satellite is of equal reflecting power with that of the planet itself. The comparison can, of course, only be effected by means of an intermediate object, seen under precisely similar circumstances with regard to the planet as the satellite itself. Such objects were the stars A and B*. A is the star which on the 2nd Sept. was mistaken for the satellite; its position then with regard to the planet was about 290°, and distance from limb about three diameters; subsequent

* The places of A and B are:

A,	R.A. = 23 12 0	P.D. = 101 57 50
B,	R.A. = 22 58 0	P.D. = 102 42 0

The above are not corrected for refraction. The star A is 10 mag. Σ , or 14 mag. h ; it is preceded, at 10 secs., on the parallel, by another equal star. There is an 8.5 Σ preceding A at about 67 secs. and 1 min. true north of it.

The star B is the larger component of a double star. Magn. 9.6, and 19 Σ . Distance circa 70'', and position about 350°. This is the true northern and faintest of two or three somewhat similar Doubles, in a field of perhaps half a degree. The errors in the places may amount to 5 secs. and 20'.

observation of the satellites caused me to estimate the star A as fainter than the satellite.

Again, on the 15th Sept., at 23 hours G.S.T., the star B was seen; its position then was about 280° , and distance from limb of planet $90''$. This star was seen in the field along with the satellite itself, and was judged to be a little fainter than the satellite. The comparison was made two or three times at short interval, so I concluded that the satellite was decidedly brighter than A, and very slightly brighter than B.

The next thing was to determine the brightness of A and B by means of graduated aperture. After several experiments I found that the mean limiting aperture for A was 2.0 inches, and for B 1.5 inches. By limiting aperture I mean the aperture just sufficient to show the star distinctly, but as the satellite was a little brighter than B, I shall assume 1.4 inches as the limiting aperture for the satellite. Now, 1.4 inch aperture, with my eye and telescope, corresponds to 9.5 mag. Σ , or 13 mag. λ , and such, therefore, I estimate the satellite to have been on the 15th Sept.

Having thus ascertained the limiting aperture for the satellite on the 15th Sept. to have been 1.4 inches, I tried to ascertain the limiting aperture for Mars and also for Vesta, which was in the immediate neighbourhood.

In the case of Mars, seen through an exceedingly small aperture, I found, as might have been expected, that magnifying power was not admissible, for it converted the enlarged disc into a nebulous haze. Therefore, in the case of Mars I employed the naked eye only. In α Lyrae, with naked eye, I found the limiting aperture to be 0.01 inch, and through this aperture I judged Mars to be a full Struvian magnitude, brighter than α Lyrae. The limiting aperture, therefore, for Mars, seen with the naked eye, would have been 0.005 inch, and in such rough estimates it seems useless to make any allowance for the amount of light lost in passing through the telescope.

The limiting apertures, therefore, for the satellite and the planet were 1.4 inch and 0.005 inch, or in the rate of 1000 to 36: if then we take the diameter of the planet to be 4,000 miles, we get 14 miles as the diameter of the satellite from this comparison.

In the case of Vesta, as with the satellite of Saturn, I found that owing to the exceeding minuteness of the discs, there was but very little loss of light consequent on using a magnifying power of 300, which had been employed on the satellite of Mars.

With this power I found the limiting aperture for Vesta to be 0.25 in. But if Vesta on 8th Oct., the day of comparison, were in the place of Mars on the 15th September, her light would be increased 45 times, and therefore her limiting aperture would be reduced from 0.25 to 0.037 inch. So the apertures for Vesta, seen under same circumstances as the satellite of Mars, and for the satellite of Mars are 0.037 and 1.4 inches, which are in the ratio very nearly of 1 to 40. So that if Vesta reflects light as does the satellite, then the diameter of the satellite would be $\frac{1}{40}$ th of the diameter of Vesta. Now, the books give the diameter of Vesta as 230 miles, which at present distance would correspond to a disc of $0''.7$, and this cannot be very far from the size of the real disc as it appears in the telescope.

Taking the diameter of Vesta as 240, we have as the diameter of the satellite, on the assumption of equal reflection, 6 miles.

It is a very curious coincidence that the mean of these two values, 14 and 6, is exactly the estimated value of the diameter given by Professor Hall. It is curious, also, how this value, 6 miles, derived from comparison with Vesta, agrees with the probable value of 5 miles, given by Mr. Proctor,

But I think it will appear that the reflecting power of the surfaces of Mars, or of his satellite, and that of Vesta are by no means equal.

For if, on the 18th Sept. Mars has been removed to the distance of Vesta his light would have been enfeebled 40 times, and therefore his limiting aperture would require to have been increased $\sqrt{40}$, or 6.4 times, and so would have become 0.032 inches, but the limiting aperture for Vesta is 0.25 inches, or only eight times that of Mars at her distance.

Therefore, on the assumption of equal reflection, her diameter should be an eighth of that of Mars, or 520 miles. Now, it is highly improbable that her diameter is more than half that quantity, and therefore her reflecting power must be four times greater than that of Mars, and if we assume the received value of the diameter of Vesta, 230 miles, to be correct, then it will appear that her reflecting power is 5.2 times that of Mars, whose diameter is 4,150 miles for $(230 \times 8)^2 \times 5.2 = 4150^2$.

Now, applying this correction to the diameter of the satellite, as determined by comparison with Vesta, we must increase the area of the disc of the satellite 5.2 times, or multiply the diameter of V5.2, which is 2.2, but $6 \times 2.2 = 13.2$, so that comparison with Vesta gave the diameter of the satellite as 13.2 miles.

Great as the disparity between the reflecting power of Mars and Vesta may thus appear to be, still it is corroborated by other observations. In the first place Vesta is known to be the brightest of the minor planets (Chambers), that is to say, her light is more intense than that of any other.

In the next place, the light of Mars is known to be less intense than that of Jupiter or Saturn, and for the purpose of getting some idea of the relative reflecting power of the surfaces of these planets, I endeavoured to compare the actual apparent brightness with their calculated brightness on the assumption of equal reflection.

On several occasions about the 5th Nov., I estimated, as carefully as my means would admit, the relative brightness of Mars, Jupiter, and Saturn. I noted the times at which the two former became visible, at nearly altitudes in the twilight; also I employed the neutral tint sun-glasses with limiting apertures, and reflection from the first surface of a prism. After many trials I came to the conclusion that Mars and Jupiter were of equal brightness, and ten times as bright as Saturn. Of course it is necessary to make some allowance for the superior illumination of the western sky, wherein Jupiter was. Suppose we assume this to have reduced his brightness one-tenth the comparison of actual apparent brightness would stand thus: Jupiter, 100; Mars, 90; Saturn, 9. It is easy to calculate the amount of light received by us, from any planet, at any distance:

$$\text{The light} = \frac{\Delta^2}{D^2 \times V^2} \times N.$$

Where Δ = Diameter of planet.

D = Distance from us.

V = Radius vector, or distance from sun.

N = Comparative reflecting power of surface.

Calculated according to this formula, and assuming equal reflection the apparent brightness would have been on 5th Nov.—

Jupiter, 100; Mars, 260; Saturn, 8.6; Vesta, 0.034.

Whereas the actual brightness was—

Jupiter, 100; Mars, 90; Saturn, 9; Vesta, 0.04.

These figures evidently agree as well as could have been expected, except in the case of Mars, whose light ought to have been three times as great as it was, whence we infer that his surface reflects only one-third of the light reflected by the surfaces of the other planets, and then the foregoing direct comparison with Vesta receives additional confirmation.

In conclusion: The diameter of the satellite, as derived from comparison with Mars himself is 14.0 miles, and from comparison with Vesta 13.2 miles, giving a mean diameter of 13.6 miles, which cannot, I think, be very far from the truth.

This at the opposition distance of Mars will correspond to a disc of 0, and at the mean distance to a disc of

Two such satellites as these, at distances from Mars of two radii and six radii, and of densities equal to that of Mars himself, would be utterly powerless to produce any tidal effect upon his seas.

Sherrington, Bray:

I am, yours faithfully,

Nov. 19.

WENTWORTH ERCK.

SATELLITE OF MARS.

Observation of one of the satellites of Mars, made with the western equatorial of the garden of the Paris Observatory, by M. M. Henry.

1877, Aug. 27th, 12h. 9m. M.T. Angle of position, $249^{\circ} 56'$. Distance from Mars $85'' \cdot 2$. Number of comp. 16.

The satellite is very faint; we only succeeded in observing it by hiding the planet behind a screen.

DISCOVERY OF A NEW ARABIC CELESTIAL GLOBE.

We read in *La Nazione*, Florence, 9th February. "We announce with much pleasure that the Royal Museum of Physics and Natural History, which forms part of our Institute of the Higher Studies, has just been enriched by an object of great value on various accounts, the discovery of which is due to the scientific activity of the Cavaliere Mencci, of the Meteorological Observatory, who is also in charge of the department of the ancient instruments of Physics. We allude to an Arabic celestial globe, the existence of which till now was altogether unknown, and is added to the few others we know of. The epoch, scientifically determined by the Cav. Mencci, is the XIth century of the vulgar era; the precise time when the instrument was completed is ascertained by the inscription (in Western (Mogrehina, qy. African?) writing, and Cufic characters with diacritical points, as in the astronomical names and the numeral letters of the globe), which has been read and translated by Prof. Cav. Fausto Lasinio, of the above named Institute. It is clear from the inscription that the author of the globe was *Ibrahim Ibu Said As-Sahli al Wazzan* (the Weigher), together with his son *Muhammad*; that the work was finished in Valenza, in the beginning of the month Safas, in the year of the Hegira, 474 (corresponding to the 11th July, 1081, A.D.); that the globe was made by *Abu Isa Ibu Labban* (or *Leban*), a person well known in the political and literary history of Mussulman Spain of that age. We know also that the Cav. Mencci is occupied with the detailed description of this globe, assisted in what relates to Oriental studies by the aforesaid Professor Lasinio."

This globe then is nearly a century and a half older than the celebrated Borgia globe, and may have been seen by the Spanish Moor, Arzachel, the author of the Toledo Tables. It is an interesting discovery.

ASTRONOMICAL OCCURRENCES FOR JANUARY, 1878.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Tues	1		Sidereal Time at Mean Noon, 18h. 44m. 0 ^h 74s.			6 0 ^h 5
Wed	2		Sun's Meridian Passage 4m. 20 ^h 52s. after Mean Time			5 58 ^h 7
Thur	3	2 3 4	● New Moon Conjunction of Moon and Jupiter 3° 8' N.			5 56 ^h 9
Fri	4	1 4	Mars at quadrature with the Sun Conjunction of Moon and Mercury 3° 34' N.			5 55 ^h 1
Sat	5	3	Conjunction of Jupiter and Sun Saturn's Ring: Major axis = 37" 29 Minor axis = 1" 83	Sun.		5 53 ^h 3
Sun	6	6 25 7 27	Occultation of 44 Capricorni (6) Reappearance of ditto	the		5 51 ^h 5
Mon	7	1	Conjunction of Moon and Venus 0° 23' S.	near		5 49 ^h 8
Tues	8	11 4 41	Conjunction of Moon and Saturn, 4° 35' S. Near approach of 21 Piscium (6)			5 48 ^h 0
Wed	9	7 31 8 41	Occultation of 25 Piscium (6) Reappearance of ditto	too		5 46 ^h 1
Thur	10	4 44 6 4 11	Occultation of 51 Piscium (6) Reappearance of ditto Inferior conjunction of Mercury and Sun	Jupiter		5 44 ^h 5
Fri	11	6 46 4	☾ Moon's First Quarter Conjunction of Moon and Mars 4° 17' S.			5 42 ^h 8
Sat	12	23	Conjunction of Jupiter and Mercury 3° 30' N.			5 41 ^h 0
Sun	13					5 39 ^h 3
Mon	14					5 37 ^h 6
Tues	15		Illuminated portion of disc of Venus = 0 ^h 274 Illuminated portion of disc of Mars = 0 ^h 880 Sidereal Time at Mean Noon 19h. 39m. 12 ^h 55s.			Capella, 9 26 ^h 9
Wed	16	6 15	Near approach of 136 Tauri (5) Sun's Meridian Passage 10m. 4 ^h 80s. after Mean Noon			9 23 ^h 1

DATE.		Principal Occurrences.	Jupiter's Satellites.	Meridian Passage.
		h. m.		h. m. s.
Thur	17	5 26		h. m. Capella.
		6 39		
		7 26		
		6 48		9 19'1
		14 2		
Fri	18	15 4		
		12 10		
		7 24		
		8 5		9 15'2
		15 31	Sun.	
Sat	19	16 31		
Sun	20			9 11'2
		11 21	the	9 7'3
Mon	21	10 16		
		10 49	near	9 3'4
Tues	22			8 59'4
Wed	23	13 29		
		13 46	too	8 55'5
Thur	24	8		8 51'6
Fri	25	3 49	Jupiter	
				8 47'6
Sat	26			8 43'7
Sun	27			8 39'8
Mon	28			8 35'8
Tues	29			8 31'9
Wed	30	13 23		
		23		8 27'9
Thur	31			8 24'0
FER.				
Fri	1	Eclipse of the Sun, invisible at Greenwich		8 20'1

THE PLANETS FOR JANUARY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	19 59 16	S. 20 21½	8".2	1 15.5
	9th	19 35 44	S. 18 56½	9".8	0 20.1
	17th	18 54 20	S. 19 31½	9".4	23 3.5
	25th	18 54 52	S. 20 41	7".8	22 32.5
Venus ...	1st	21 55 27	S. 13 5½	32".0	3 10.9
	9th	22 16 32	S. 9 57	36".0	3 0.5
	17th	22 31 54	S. 6 57½	40".8	2 44.4
	25th	22 40 3	S. 4 22	46".4	2 21.0
Mars ...	1st	0 45 31	N. 5 8½	10".2	6 0.5
	9th	1 2 44	N. 7 7	9".6	5 46.2
	17th	1 20 28	N. 9 4	9".0	5 32.5
	25th	1 38 40	N. 10 58½	8".4	5 19.2
Saturn ...	1st	23 9 26	S. 7 37½	15".0	4 24.7
Uranus ...	5th	10 5 41	N. 12 33	4".2	15 3.4
	17th	10 4 13	N. 12 41	4".2	14 14.8
Neptune ...	1st	2 12 34	N. 11 26	...	7 27.3
	17th	2 12 22	N. 11 26	...	6 24.2

Mercury may be observed an hour and a half after sunset on the 1st, the interval decreasing. In the middle of the month he rises about an hour before the sun, the interval increasing.

Venus sets four hours after the sun, at the beginning of the month, the interval decreasing to three hours.

Mars is visible till about half-an-hour after midnight throughout the month.

Saturn sets about a quarter before 10 p.m. on the 1st, and then earlier each night. On the last day he sets three hours and twenty minutes after the sun.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN JANUARY, 1878.

By W. R. BIRT, F.R.A.S., F.M.S.

In our list for November, 1877, p. 302, it was stated that B. and M.'s *f* is situated in area II. L. v, it is registered as II. L. v 1. In this area a ridge, II. L. v 2, crosses the south part of the floor of J. J. Cassini; it is not given by Schröter, but is plainly shewn by B. and M.; so far as it has been observed it appears to close up, or nearly so, the valley at its south end which is left open by Schröter; this ridge may be found by aligning with the west wall of Herschel II., and also with the crater Horrebow.

The south part of the east border of J. J. Cassini is registered as II. L. ° 3; several small craters on and near the valley have been observed which are neither in Schröter nor in B. and M. As a guide to these small craters we take Schröter's *p* = B. and M.'s Fontenelle *C*, the meridian of 25° E. long. passes through the western of the

two craters marked respectively p and C , it is consequently registered II. I. $^{\circ} 1$; II. K. λ 2. South of Schröter's p we have his ζ II. I. $^{\circ} 6$, described in the register as a *small* crater. The six small craters entered only in the British Association Register are as follows: II. I. $^{\circ} 4$ south of the western of Schröter's two craters at p on the *west* side of the border of J. J. Cassini; II. I. $^{\circ} 5$ also south of Schröter's p and *west* side of border; II. I. $^{\circ} 7$ south-west of p , but on the *east* side of border; II. I. $^{\circ} 8$ north-west of p , this craterlet aligns with p and ζ ; II. I. $^{\circ} 9$ W.N.W. of p on east of border; II. I. $^{\circ} 10$ is described "on the border," an object north of it is not given; Schröter's two craters at m , S.E. of Fontenelle are registered respectively II. I. $^{\circ} 2$ and II. I. $^{\circ} 11$. The following notes have reference to the seven craters. II. I. $^{\circ} 4$ to 10. II. I. $^{\circ} 4$ and 5 are very close together west of II. I. $^{\circ} 6$ the ζ of Schröter's, which forms a small group with them. B. and M. shew a small crater in the locality of this group.

On Schröter's drawing from the mouth of the valley towards the south the east border is continued as far as ζ by a curved ridge, the convexity of the southern part being directed towards the east, and is registered II. I. $^{\circ}$ 12, II. I. $^{\circ}$ 13 the south end, II. I. $^{\circ}$ 14 the north end. The ridge is well shown by B. and M. who mark the south end χ and the north λ .

Mr. Dennett on page 331, vol. XV., calls attention to a ridge at the northern extremity of the eastern wall of Fracastorius, which he considers to be the eastern wall of an *immense depression*. When the terminator approaches very closely to an object the hypsometrical characteristics are much exaggerated, it is possible that Mr. Dennett's *immense depression* may be nothing more than the increasing shadow of the ridge in question falling on a surface slightly inclining towards the ridge; it is about one day after or one day before the terminator has passed, or is about to pass, over a number of objects that they are seen to the best advantage, and their true characters are best appreciated. To test Mr. Dennett's observations and ascertain its true significance, a series of observations during 1878 would be valuable, every alternate month the terminators return to nearly the same localities on the moon's surface. Taking the epoch of Mr. Dennett's observations, 1877, September, 26, 11 o, the most suitable times for observing his objects in 1878 are as follows:—

Jan.	21	11	0	March	21	11	0	May	19	11	0
"	22	11	0	"	22	11	0	"	20	11	0
July	17	11	0	Sept.	14	11	0	Nov.	12	11	0
"	18	11	0	"	15	11	0	"	13	11	0

At each of these epochs the objects will be removed further from the terminator, on Jan. 22 three hours must elapse before the terminator will occupy the place it did on the previous Sept. 26, 11, and on Nov. 13, 1878, as many as 101.

Buckhurst Hill: December 14, 1877.

Erratum.—November list, last line but four, for *Isola* read *Iota*.

SATELLITES OF URANUS.

The major and minor semi-axes a and b of the apparent ellipses described by the satellites are, on Jan. 25:

Ariel.		Umbriel.		Titania.		Oberon.	
a	b	a	b	a	b	a	b
15° 18'	4° 50'	21° 14'	6° 26'	34° 68'	10° 27'	46° 38'	13° 74'

The position-angles of the major axes are 8° 13' and 188° 13', those of

The position-angles of the major axes are $8^{\circ}.13$ and $188^{\circ}.13$, those of

the minor axes at inferior conjunction $278^{\circ}13$, and at superior conjunction $98^{\circ}13$, the satellites moving so that their position-angles decrease, or the direction of their apparent motion is that of the hands of a watch. The satellites will be at their greatest elongations N. and S., and at their sup. and inf. conjunctions with the planet, about the following hours, G.M.T.:

Ariel.		Umhriel.		Titania.		Oberon.	
1878.	h.	Jan. 20.	h.	Jan. 20.	h.	Jan. 20.	h.
Jan. 20.	12 S.	21 N.		19 S.		22 S.	
21.	18 N.	22.	23 S.	22.	23 sup.	24.	7 sup.
23.	0 S.	25.	0 N.	25.	4 N.	27.	16 N.
24.	7 N.	27.	2 S.	27.	8 inf.	31.	1 inf.
25.	13 S.	29.	4 N.	29.	12 S.	Feb. 3.	10 S.
26.	19 N.	31.	6 S.	31.	16 sup.		
28.	1 S.	Feb. 2.	7 N.	Feb. 2.	21 N.		
29.	8 N.						
30.	14 S.						
31.	20 N.						
Feb. 2.	2 S.						

Observers will have a convenient opportunity for ascertaining whether they can discern Uranus with the naked eye, as the place of the planet may be easily found in the neighbourhood of α Leonis. On Jan. 21, it is $26'$ to the East and $11'$ to the North of the star, on Jan. 31, $3'$ to the East and $19'$ to the North.

A. M.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1877.		To Nov., 1878.		Hall, Rev. R.	
Brisson, J. R.		Herschel, Major J.		Herschel, Prof. A. S.	
Dausken, J.				Johnson, Rev. S. T.	
Hubbersty, Rev. R. C.				Jones, Rev. E.	
To March, 1878.		To Dec., 1878.		Joynton, J.	
Harker, J. C.		Baldelli, Contessa.		Knott, G.	
		Barber, J. T.		Lambert, O.	
		Bernserts, G.		Richards, E. M.	
		Boë, A. de.		Rogerson, G. R.	
		Duncan, J.		Smith, Dr. R.	
		Goode, J. F.		Tidmarsh, Rev. J. B.	
		Fisher, J. S.		Walker, G. J.	
		Franks, W. S.		Williams, G.	
To Sept., 1878.					
Lettsom, G. W.					

TO CORRESPONDENTS.

Mr. Nelson's article on Jupiter is unavoidably postponed.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will at once inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or penny postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for the Editor are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The *Astronomical Register* is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, payable in advance, by postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, Angel Court, Throgmorton Street, E.C., not later than the 15th of the Month.

The Astronomical Register.

No. 182.

FEBRUARY.

1878.

ROYAL ASTRONOMICAL SOCIETY.

Session 1877-78.

The Third Meeting after the long vacation, Friday, January the 11th, 1878.

William Huggins, Esq., D.C.L., LL.D., &c., *President*,
in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

The following gentlemen were elected Fellows of the Society:

Rev. William H. Allen, 3, Kelly Street, Kentish Town Road, N.W.

T. H. E. C. Espin, Esq., Wallasey Rectory, Birkenhead.

Rev. David Fleming, Coxhoe Vicarage, Co. Durham.

F. R. Janisch, Esq., C.M.G., Governor of St. Helena.

Lieut.-Col. H. S. Knight, Army and Navy Club, S.W.

Albert Marth, Esq., 66, Lambeth Road, S.W.

George Francis Riddiford, Esq., Barnwood Lodge, near Gloucester.

Rev. Philip R. Sleeman, Clifton, Bristol.

Rev. G. E. Watts, Kensworth Vicarage, Dunstable.

Mr. Glaisher said: Forty-seven presents have been received since the last meeting; the only ones which need special mention are five drawings of the eclipse of the moon on August 23, 1877, made by the Rev. S. J. Johnson, and a diagram showing the distribution of the stars of the southern hemisphere, which has been given to the Society by Mr. Sidney Waters.

The thanks of the Society were returned to the donors.

The President said: This is the evening on which it is usual to appoint auditors to audit the accounts of the Society before

VOL. XVI.

the General Meeting in February. Perhaps someone will propose the names of some auditors.

Mr. Lecky: I have much pleasure in proposing the names of Mr. A. Common, Dr. Wentworth Erck, and Mr. Kennedy Esdaile.

Mr. Barrow having seconded the motion, it was carried without opposition.

Mr. Ranyard read a paper by Mr. Denning *On Suspected repetition and second outbursts from radiant points, and On the long duration of meteor showers*. The paper stated that the radiant points referred to had been reduced from the observation of 2,690 shooting stars, which had been observed by Mr. Denning between April, 1876, and December, 1877, and also from the catalogue of the Italian Meteoric Association. The result obtained appeared to show that many showers have very long durations, in some cases lasting for three or four months. There appeared to be a tendency in many radiants to repeat themselves and give secondary outbursts at intervals of three months, although there was no actual cessation. A list of some twenty such radiant points were given in the paper.

Captain Tupman having been called upon said: I am afraid this is somewhat a dry subject; but at any rate Mr. Denning's results are exceedingly remarkable. In this paper he gives about twenty-two radiants, apparently lasting for three months, some of them continuous all that time and others starting or breaking out again; but he uses the term "shower" for the whole period. The first question raised is, what is a shower? I suppose a shower of meteors is caused by a number of small bodies moving in very nearly the same orbit. The breadth of space occupied is open to a little question, but it cannot be very great, and it will be evident that in the case of meteor showers making a considerable angle with the plane of the earth's orbit, the earth must pass through them in a few days at most. The only case in which the earth could remain longer than a few days in a meteor shower is when the perihelion distance is equal to the earth's distance from the sun, and the plane of the orbit coincides or makes only a small angle with the ecliptic. Looking through Mr. Denning's list, only two or three of the showers fulfil these conditions, and they would have, perhaps, from three to six weeks' duration, but three or four months' duration is absurd, even for such showers. There is a great deal of use, however, in this paper, because the method Mr. Denning has employed is that begun by Heis and pursued by Greg, Alexander Herschel, Schiapparelli and others. It consists in plotting on the same chart the tracts recorded on a great many nights and deriving at once the radiants and their durations; and it was, indeed, the

only way to treat a comparatively small number of observations. Now-a-days we might keep every night of the year separate. In other words the *Node* should be considered as equal in importance to the radiant point. It is still better to collect duplicate or multiple observations of the same meteor by a concerted plan of action. Such radiants were free from every objection. This method is much more exact than the other. If a great number of observations of that kind could be collected I have no doubt that these agreements of Mr. Denning would disappear, because it is quite absurd that a shower, properly so called, should last so long. At the same time he is quite right to publish his results, and no person who has not undertaken such a work can know the labour of mapping several thousands of these shooting stars. There is one point I should like to mention, and that is, that if anyone were to make a mark upon any part whatever of a globe representing the heavens, you could always find meteors whose paths would appear to pass through it. Three or four such meteors could be selected on any rich night, and one or two on a poor night; so that you could establish that that point was a radiant point lasting the whole year round. That is of course an extreme case. Many of these determined by Mr. Denning are, I expect, pseudo-radiants, and I am sure Prof. Herschel is of the same opinion; two or three of them, perhaps, coincident with the plane of the ecliptic, may last a month or more.

Mr. Ranyard said: Mr. Denning has in a letter to me thrown out the suggestion that these persistent showers may be due to streams of meteors which are connected with the orbit of the earth. It will be remembered that there are families of comets whose orbits are connected with the orbits of the larger planets. There are several comets which form, for example, a Jovian family, and others which are connected with the orbit of Uranus. Mr. Denning's idea seems to be that these meteors correspond to comets which are connected with the earth's orbit, and that the earth might thus be immersed for many months in streams which happened to make only a small angle with the plane of the earth's orbit. But it will be evident that the radiant points corresponding to such streams would shift as the earth proceeds in its orbit, and the meteors would appear to come from quite a different part of the heavens after a lapse of three months.

With regard to the diameter of meteor streams, if some of the matchings which have been relied upon as showing a connection between cometary paths and radiant points turn out to be more than mere coincidences, it will follow that the diameter of some of the streams must be very great. For example, Mr. Denning thinks that he has found a stream corresponding to

Donati's comet, but if I remember rightly the orbit of Donati's comet does not approach within a distance of forty millions of miles of our own orbit, and consequently the stream of meteors corresponding to the comet must be at least eighty millions of miles in diameter. If we went diametrically through such a stream, supposing the plane of our orbit to cut the stream at right angles, it would take us at least forty days before we got clear of the stream; for the earth does not move through two millions of miles in a day. But the meteors which we should encounter on first entering such a stream would not be moving parallel to the meteors encountered on the other side of the stream, for they would all be moving in orbits having the sun in the focus, and consequently the planes of the orbits must be inclined, and the meteors seen when we first entered the stream would not appear to have the same radiant point as those seen just before we passed out of it. It seems, therefore, impossible to conceive* of a radiant point remaining fixed for months, and the whole question seems to turn, as Captain Tupman has pointed out, upon the degree of accuracy with which these radiant points of Mr. Denning have been determined.

The thanks of the Society were voted to Mr. Denning for his paper.

Mr. De la Rue suggested that Captain Tupman should be requested to embody his criticisms in a communication to be published with Mr. Denning's paper in the *Monthly Notices*.

Mr. Ranyard said that Captain Tupman had already promised him to do so.

Dr. Wentworth Erck explained an instrument which he had devised for dispensing with the use of a declination circle on a large Newtonian Equatorial. It consisted of a divided circle, with a spirit level, which can be fixed to the adapter in which the eye-piece slides. The telescope is brought to the meridian, and is then set to the declination of the object required by means of the circle and level. The instrument can also be used as a position circle for the measurement of double stars.

Dr. Erck also showed a small and very compact equatorial mounting which he had devised for attaching to the edge of a

* [Note by Mr. Ranyard.] It is possible to conceive of radiant points remaining stationary, or nearly stationary, during the whole year if there are sporadic meteors which the earth and planetary system in its motion through space encounters. But the velocity of such meteors in space would have to be very great compared with the velocity of our earth in its orbit—or there would be a very sensible motion of the radiant point as the earth passed round the sun—and this theory will not of course account for the second outbursts and intermittent showers observed by Mr. Denning.

window sill, or in any other convenient position, by means of a clamp. He said that he had been requested to mention some improvements which had been introduced in the construction of a spectroscope by Mr. Grubb, of Dublin, now making for Professor Young. With a spectroscope, as ordinarily used, it is usual to adjust the collimating lens so that the pencil of rays incident on the first prism shall be parallel for rays of mean refrangibility; and when you go to the red end, or to the violet end of the spectrum, you adjust the eye-piece for the best definition. But in this manner it is not possible to get the best definition. It is evident that both collimator and eye-piece ought to be focussed. In Mr. Grubb's instrument this is effected thus: the rays, after passing through five prisms, suffer two internal total reflections, and emerge parallel to the incident pencil, but a little on one side. They are then received by the observing telescope, which is placed parallel to the collimating telescope, and the eye-piece of the observing telescope is fixed by a rack motion to the collimating lens, so that the motion of one pinion screw focusses the collimating lens and the observing telescope at the same time. Another improvement is that the axis of the collimating telescope is coincident with the axis of the great telescope, and the limb of the sun's disc can be brought opposite the slit by means of two reflections from the surfaces of a glass parallelopiped, whose length is equal to the radius of the disc; thus by merely revolving the parallelopiped, without displacing the spectroscope, any part of the sun's limb can be brought upon the slit.

Mr. Browning: It appears to me that this method of Mr. Grubb's is exceedingly ingenious, but it is a mistake to suppose that spectroscopes have not been made before with an arrangement for focussing the lens of the collimator. I am in the habit of making a form of spectroscope which you can put in your pocket, and in which there is a separate sliding draw tube attached to the slit, so that the distance between the slit and the collimating lens can be varied for different parts of the spectrum. In fact I have been in the habit of providing for the focussing of the slit in nearly all my spectroscopes. And in one I made twelve years ago for Mr. Gassiot, there was a rack-work motion for moving both collimating lens and eye-piece by a scale. Then with regard to the method of mounting the spectroscope on the side of the large telescope, some two or three years since I had to make a reflecting telescope 15 inches in diameter, and I did not attempt to put the spectroscope, which was very heavy, on the side of the telescope, but I laid the axis of the collimator parallel to the axis of the large telescope, and

I applied a large prism, and by its means threw the ray into the collimator, and by the rotation of the prism I got the same result as if I had rotated the spectroscope.

Dr. W. Erck: As soon as the collimating lens and eye-piece of the telescope have been adjusted for rays of mean refrangibility, there is with Mr. Grubb's spectroscope no other adjustment necessary.

Lord Lindsay: I think Mr. Browning hardly followed Dr. Erck's meaning. I understood him to say that by focussing the telescope you moved the collimating lens by the same motion.

The President: I am sure I shall anticipate your wishes by awarding your thanks to Dr. W. Erck for what I may call, perhaps not in very scientific language, but in an expressive form, these "ingenious dodges" which he has brought before us. I think that both Dr. Erck and Mr. Browning are right in what they have stated. It is, I think, quite true, as Dr. Erck stated, that usually observers with the spectroscope do not take into consideration the fact that the collimating lens requires focussing for the different parts of the spectrum. At the same time, I remember distinctly what Mr. Browning states as to the large instrument made for Mr. Gassiot, that it had this rack adjustment for the collimator. As a spectroscope observer, I have always taken into consideration that the collimating lens required focussing for the part of the spectrum in which I was observing. At the same time I think it would be a considerable advantage to be able to focus the collimator automatically, as proposed by Mr. Grubb.

Mr. A. A. Common read a paper describing some observations he had made with an 18 inch silver on glass reflector, by Calver, of the satellites of Mars. He had upon nine different nights succeeded in seeing, and in most instances in measuring, the position of the outer satellite. This he was only able to accomplish by hiding the planet by means of a sliding bar. The colour of the satellite appeared to him to be ruddy, like that of the planet.

Mr. Ranyard read a note which he had received from the Astronomer Royal mentioning that the observers at Melbourne were not certain that they had succeeded in observing the satellite of Mars with the great Melbourne four-foot reflector. When the news of the discovery first arrived, the declination axis of the great instrument was broken, and during the time of the opposition of the planet they were only able to search for the satellite with an 8-in. silver on glass reflector. They entirely failed in observing the satellite with the smaller instrument, and only succeeded in observing it upon one occasion, about which there

seemed to be some doubt, with the larger instrument. This is the more remarkable, when we remember the favourable geographical position of Melbourne, which would bring the planet near to the zenith at the time of opposition.

Mr. Neison said that he understood that on the 15th September Mr. Common had seen a small star which was not so bright as the satellite. He wished to ask whether Mr. Common had any idea of its apparent magnitude? Dr. Erck had estimated the star as of the 13 magnitude of Herschel, which would correspond to the $9\frac{1}{2}$ magnitude of Struve. He believed the brightness of the satellite had been estimated as corresponding to the 14th magnitude by Professor Hall and other American observers, but they do not state what magnitudes they referred to, whether those of Herschel or Struve.

Mr. Common said he was "quite at sea" in estimating magnitudes with the telescope he had used. But the magnitude of the two stars preceding the planet on the night of the 15th September were such that they might be well mistaken by some observers for the satellite itself. One referred to as B in the paper by Dr. Erck was spoken of as brighter than the satellite, but it appeared to him infinitely brighter.

Dr. Erck said he had observed the same pair of double stars. His observation referred to the brighter of the two double stars, which he carefully measured. He had a letter from Professor Hall stating that it was singular the inner satellite was not seen in Europe, as it was brighter than the outer one, and it had been observed in America within ten seconds of the planet's limb.

Mr. Christie said: As far as I can judge from what I have seen of the outer satellite, it strikes me the estimate of the American observers is very much smaller in magnitude than I should be inclined to make it. I thought that it was certainly about the 13th magnitude. It was most distinctly visible when you could catch it with a 13 inch equatorial, but, at the same time, you might easily pass it over. With regard to the inner satellite, Mr. Maunder, I believe, has seen it once, but the great difficulty has been to catch it at its elongation. The difficulty is not its faintness, but the rapidity of its motion. It only remains in a position where it can be seen for some two or three hours at a time.

Dr. Erck: On the other hand, Professor Hall says it is visible to within 10 seconds of the limb.

Mr. Christie: But in a couple of hours it may become cloudy; and you have to make your observations when you can.

Dr. Erck: If I might say a word with regard to Mr. Common's telescope, we can judge of its perfection from what it has done.

The remarkable thing about it is, that the focal length is not six times the diameter of the speculum—i.e., the speculum is 18 inches, and the focal length is less than 9 feet. It is accommodated in a small shed, such as a country carpenter might make. If you had a refracting telescope that would do as much, you would have an aperture of say 16 inches, which would require a focal length of 20 feet, which means a dome of 30 feet diameter, and all the other expenses of construction increased in the same proportion.

The President said: I think a great many of these differences in judgment with regard to the visibility of faint objects with different telescopes are far less interesting and important than many persons suppose. So much depends on a clear night. At times I have seen an object as clearly again as at other times with the same instrument. The extraordinarily clear state of the air which very often exists after a wet day may enable an observer to see with an ordinary instrument what in another locality one might fail to see with a larger instrument.

Mr. Waters was called upon to read a paper *On the distribution of the fixed stars of the southern hemisphere in space*. He had, from the star gauges of Sir John Herschel, published in the Cape of Good Hope observations, constructed a map of the southern hemisphere on the equal-surface projection, putting in stars to correspond to each of the gauges given by Sir John Herschel, and filling up the interspaces with stars according to a law of averages derived from the surrounding gauges. The position and outline of the Milky Way came out much more strikingly than in the case of Mr. Proctor's map of the northern hemisphere, but it must be remembered that the Bonn catalogue, from which Mr. Proctor's map was constructed, was made with a telescope of only 2½-inches in diameter, whereas Herschel's star gauges were made with a reflector of 18½-inches diameter. Mr. Waters pointed out that the distribution of the stars upon the map showed that the stars were distributed very irregularly in space, and that no assumption, such as that made in Mr. Stone's paper, which supposed them to be distributed with a degree of uniformity in the plane of the Milky Way would account for the clusters and streams which appear upon his map. He thought also that the map showed evidence of the great difference in the actual magnitude of stars.

The President: I think it is quite in harmony with what would be expected from physical reasons that the stars should not present uniformity in intrinsic brightness.

The thanks of the Society were voted to the reader of the paper.

Mr. Christie, who was called upon to read a paper *On the specular reflection of Venus*, said: It will be remembered that there has been some question raised as to whether there is a certain gradation of light on the disc of Venus which may be referred to specular reflection or not. About the end of 1876 I examined the disc of Venus, and found there was a bright spot of light, and the position of that spot agreed pretty well with the position which would be assigned on the theory of specular reflection, but at that time the disc was somewhat gibbous, and therefore the experiment was not crucial, and if there were a considerable error made in estimating the position of the spot it might be accounted for on the ordinary theory of light diffused from a tolerably rough surface. Venus is now in a more favorable position, and while in the crescent form I have repeated my observations. If there is specular reflection the maximum brightness should be a little within the limb, whereas if the illumination is due to diffused light from a rough surface the greatest brightness would be at the limb, the maximum being on the other side of the limb which we cannot see, so that we should see the light increasing right up to the limb. I have made some observations on this point and have had the co-operation of Captain Tupman on two occasions, and he will bear me out in what I saw. I do not see at present that the appearance can be explained on any other theory than specular reflection. We have seen exactly what Mr. Brett described some time ago, which is that the limb of Venus is not the brightest part, and that it shades off slightly towards the limb. (Here Mr. Christie drew on the black board a diagram of the planet showing the part which last disappeared.) I have satisfied myself that the brightest part was not on the limb. Captain Noble some time ago brought forward an observation of Venus, which he said he had made with a dark glass. According to him all the limb and all the part near the terminator went out as the light was reduced, and there was nothing left but a patch between the two; there was not the slightest degree of gradation. Now, if the patch were due to any ordinary dispersion of light from a rough surface, there would be considerable gradation. If there is specular reflection the brightest part would have been at the time that I examined the planet $\frac{2}{10}$ ths of a radius from the limb. I am convinced that such a bright patch did exist, and that there is a very decided gradation on either side, and I can see no other way in which such a patch could be accounted for except on the theory of specular reflection. I made the observations with my polarising eye-piece which I have previously described.

Mr. Banyard: I think we are not necessarily driven into the

specular reflection theory by the observation of Mr. Christie that the brightest part of the disc is not adjacent to the limb. If we suppose the body of Venns to be surrounded by an absorbing atmosphere and the clouds or the surface which reflects the light to us to be situated at some distance beneath such an atmosphere, then the absorption would be greatest at the limb of the planet, so that in considering the brightness of any part of the disc there will be two factors to take into account—first, the altitude of the sun as seen from any part of the disc of Venus, which will cause the brightness to increase from the terminator to the part of the planet where the sun would be in the zenith; and, secondly, there will be the absorption of the ray in the atmosphere of Venus. This would be greatest at the limb, and thus there will be a point intermediate between the limb and the terminator, where for any particular law of absorption the brightness of the disc would be greatest; but my chief reason for not believing in the specular reflection theory is that I have examined the light of the planet with a bi-quartz polariscope and with a Savart polariscope, each of them instruments which would detect a very small proportion of polarized light, but I have not found the slightest trace of polarization. This is rather remarkable when we remember that traces of polarization may be detected with similar instruments from nearly every terrestrial object. The light reflected from leaves of trees, from the grass upon a grass-plot, and even from a ploughed field, shows very distinct bands with a Savart polariscope. There is hardly any terrestrial object you can turn to, except a low-lying clond, which will not show distinct traces of bands, and therefore no doubt our earth when seen from a distance, especially when we take into account the surface of the sea, would show strong traces of polarization, bnt the planet Venus shows none.

Mr. Brett: This test that Mr. Ranyard has brought to bear is no doubt mnch more sensitive than Mr. Christie's. He nes the term "sea" and says yon can even see specular reflection on the sea. You can seldom see anything else bnt specular reflection from the sea!

Mr. Neison: Allow me to give my opinion on the matter. If Venns were made of plaster of Paris one would expect the brightest portion would be at the limb; but within an atmosphere there would be a greater amonnt of absorption of the light close to the limb than there would at a little distance within it. To estimate the amount of that difference one may take the amount of absorption which exists in our own atmosphere, when you see an object near the zenith and again at a distance of 45 degrees from the zenith and lastly when near the horizon. The amount of light absorbed when the object is at 45 degrees from the zenith is

about $\frac{1}{10}$ ths of the total amount of light, whereas when it is within 10° from the horizon it is rather more than $\frac{2}{3}$ ths of the light. Thus, though the region near the limb might be bright, it would not look so from the loss of light by absorption. There is another reason which will account for the brightest part of Venns not being close to the limb. When you observe a bright object, the portions near the limb are spread out and form a spurious disc, and there is a spurious fringe on the limb of Venus which may be taken probably at a second of arc in breadth, if we may judge from the measures of Plummer. The irradiated fringe would decrease rapidly in brightness from the real edge to the extreme border. Now Venus, I suppose, at the time this drawing was made, had a diameter of about 33 seconds of arc from pole to pole. (Here Mr. Neison went into a calculation to show that the brightest part of the disc would upon the irradiation theory be at about $\frac{2}{10}$ ths of a radius from the limb.)

Mr. Christie: I am obliged to Mr. Neison, but I am sorry to have to upset his calculation. The irradiation amounted to nothing, or next to nothing, because by reducing the light the irradiation must have vanished.

Mr. Neison: You cannot get rid of the spurious disc.

Mr. Christie: If you observe 12th or 13th magnitude stars you would not find much spurious disc about them. If you reduce the brightness of an object sufficiently irradiation will be insensible. I think it is hardly fair to bring two different theories to bear first on one part of my observations and then on another. When I observed the spot of light condensed to a point when the planet was gibbous I was told that by still further reducing the light I should find it nearer the limb, and now I am told there is no condensation to a point, but that the absorption of the atmosphere gives some other form of condensation. Of course I cannot anticipate all objections; but the phenomenon is as I have drawn it. It is all very well to assert that there is a slight gradation on the limb and refer it to absorption of an atmosphere (although I am rather inclined to dispute the possibility of any absorption affecting the phenomenon to an appreciable extent) and if you admit absorption, then that will not explain the bright points, and the rapid condensation of light happens to be just where specular reflection would take place, and unless some explanation of this can be given I should be inclined to accept the specular reflection hypothesis as most probable. The polarisation argument amounts to nothing, because polarization from a metallic surface is elliptical and almost nil, so that the light of the planet would probably not appear sensibly polarized, and the polarization test is far too difficult to apply in a case of this kind.

Mr. Ranyard: Do I understand you then to adhere to Mr. Brett's theory of the mercurial surface? Even such a surface would give you polarization which would be very sensible.

Mr. Christie: I incline to the theory of a metallic surface. I do not say a surface of mercury.

Mr. Bidder: Do I understand Mr. Christie to say that by diminishing the light you reduce the diameter of the spurious discs?

Mr. Christie: Most distinctly.

Mr. Ranyard: Mr. Bidder, I think, means the spurious disc and Mr. Christie means irradiation.

Mr. Christie: Excuse me. I am speaking of the diffraction disc as formed by the object-glass with its aperture reduced. Every one knows that when you look at a faint star the disc is sensibly a point. I do not think we have any right to assume that the irradiation is larger than that point when you have reduced the light to a minimum.

Captain Noble: I should like to recall to Mr. Christie's attention the fact that when last year he corroborated what Mr. Brett saw, he described a "sausage-shaped area" of greater brightness. I should like to know how upon the specular reflection theory he can account for that?

Mr. Brett: Allow me to say that the "sausage" was the invention of Captain Noble.

Mr. Common: There is one point that has struck me. I should like to ask whether it is not possible to make an object-glass or speculum that will condense rays towards the centre of a planetary disc and would show the appearance that has been described. I cannot help thinking that it may due to some such imperfection in the instrument used.

Mr. Brett: Did Mr. Ranyard say the absorption would be greatest at the limb?

Mr. Ranyard: I did; but I should like to mention some measurements made by Dr. Draper of New York. He found that the absorption of our atmosphere is so great that it took 100 times as long to take a photograph of the ultra-violet parts of the spectrum at sunset as it took to obtain the same photograph when the sun was on the meridian, showing the enormous amount of absorption that takes place when the sun is near the horizon, and the atmosphere of Venus may be much deeper than that of the earth. And with regard to the matter of irradiation I should like to point out that there is a physiological irradiation as well as a physical irradiation, by which I mean the irradiation or enlargement of the image produced by the spurious discs, which can be explained by the undulatory theory. This physiological

irradiation is well known. Anyone who has seen an incandescent platinum wire must have noticed how it appears to swell up and become thick as the heat increases, so that the wire looks at last like a thick white string. The theory I believe is that when any of the rods and cones of the retina are very violently agitated, the adjacent rods and cones are in some way disturbed, so that the effect of light is produced over a larger area than that which corresponds to the luminous object. I do not know to how much this irradiation may amount, but probably to many minutes, possibly 1° . It is a much larger matter than the physical irradiation.

Mr. Brett: I only wish to ask Mr. Ranyard one question. Whether the absorption he speaks of was at the limb of Venus or at the bottom of our atmosphere? He was speaking of the absorption of the earth's atmosphere, which was nothing to the purpose.

Mr. Christie: About the physiological irradiation I think there is a misapprehension. Physiological irradiation amounts to not more than half a minute of arc. I used a power of 500, therefore the effect on Venus would be something between a 20th and a 15th of a second of arc. There is a general rule with regard to physiological irradiation, that it only takes effect when the emergent pencil is greater than the pupil of the eye, so that when a sufficiently high power is used irradiation comes solely from the telescope, and the physiological irradiation may be left out of the account when using a sufficiently high power.

At the instance of Mr. De La Rue,

Mr. Christie said: Mr. De La Rue has suggested that I did not explain how the light of Venus was reduced. I have done this in my paper, but I should have mentioned that it was done by reflection of light three times from plane glass surfaces in the eye-piece, and therefore a full aperture was used, yet the light was reduced to $\frac{1}{3000}$ part. That may remove some difficulties as to irradiation.

The President, in proposing the thanks of the Society to Mr. Christie, said: I would wish for a moment just to state what possibly may be of interest to some of the members. I have been asked to mention that the Kew Committee are anxious to find some gentleman of sufficient ability to go out to the Mauritius as first assistant to the Meteorological Observatory there; and some of you may know some gentleman who would be willing to go. The salary is £200 per annum and free quarters.

Mr. Glaisher read a paper by Prof. B. A. Hall, *On the centre of gravity of the apparent disc of a planet*. The paper contained

formulæ giving the position of the centre of gravity, in terms of certain quantities, both when the planet was crescent and gibbous. These were of use in reducing observations where the apparent disc was bisected.

Mr. Marth mentioned that Bessel had published in the *Astronomische Nachrichten* a paper with a similar object.

Mr. Chambers asked when a certain quarto volume of a certain quarto series would be issued to the Fellows?

Mr. Ranyard said that 480 pages of the Eclipse volume would be issued to the Fellows before the February meeting. The remaining part of the volume, which would amount to about 200 pages, would, he hoped, be issued in the course of a couple of months.

The meeting adjourned at 10 o'clock.

CORRECTION.—We are requested by the Rev. T. W. Webb to say that the curious old print of the Path in the Shadow Eclipse of 1724 was given to the Society by him, and not by Mr. Peyton, as stated by Mr. Glaisher.

ON THE ATMOSPHERE OF JUPITER.

BY E. NEISON.

(Continued from Vol. XV., p. 262)

In the preceding pages it has been shown that any atmosphere to the planet Jupiter, if composed of gases similar in density to those constituting our own, must very rapidly increase in density as its depth beneath the uppermost strata increases. In round numbers the density may be said to be increased tenfold for every 20 or 25 miles beneath the uppermost strata of the atmosphere. The effect of the presence in the atmosphere of Jupiter of some hypothetical new gas of very slight density has been considered, and it has been shown to be immaterial whether such a gas exists or not, for it would be without influence on the problem to be determined, which, be it remembered, is to ascertain the depth to which the atmosphere of Jupiter extends beneath the uppermost cloud-bearing strata.

Suppose that the planet possesses an atmosphere, what would be the phenomenon it would cause during the occultation of a satellite? When the satellite in its slow motion towards the planet, reached the point where its rays commenced to cross the outermost strata of the atmosphere of Jupiter, it would commence to be retarded in its apparent motion. For in traversing the atmosphere the beam of light would be refracted. As the satellite

progressed in its path its motion would gradually become slower and slower, because the refraction that the pencils of light would undergo would increase as the amount of atmosphere which it was compelled to cross would become greater and greater. Before long the form of the satellite would gradually alter, and become compressed in the direction of its motion, and elongated in a direction at right angles to this. For the satellites of Jupiter possess real discs, and even were there no irradiation or diffraction they would appear as circular discs about one second of arc in diameter. Therefore, the rays of light reaching the earth from the following limb of the satellite would enter the atmosphere of Jupiter over 2,000 miles further from the centre of the planet than the parallel corresponding rays from the preceding limb of the satellite. Now, whatever may be the density of this atmosphere at any point, at a point 2,000 miles beneath it unquestionably the density must be very much greater, for the air at this point would be still further compressed by the additional weight of 2,000 miles of atmosphere above it. In fact it has been shown already that were the supposed atmosphere composed of gases similar in density to our own, the density at the latter point would be millions of times denser. Indeed, such is the great power of gravity on Jupiter, were the constituents of the atmosphere ten times lighter than air, the same statement would also hold true. Be the increase of density what it may, the refraction undergone by the rays in passing through the atmosphere would be increased in a corresponding manner. These rays would, therefore, no longer reach the observer. The pencil of light which would reach the observer from the preceding limb of the satellite would be those which crossed the atmosphere at a distance beneath the path traversed by the rays from the other limb of the satellite, such that the refraction they underwent would be equal to the apparent diameter of the satellite. As seen from the earth, this point would not be so far from the point from which seemed to come the rays from the following limb of the satellite, as the real geocentric diameter of the satellite. Therefore the satellite would appear to be compressed. There would occur, in fact, exactly the same phenomenon as takes place during a terrestrial sunset. The solar disc seems flattened, because the rays from the limb nearest the horizon are refracted more than those proceeding from the limb farthest from the horizon. From a perfectly analogous reason the diameter of the satellite at right angles to the flattened one would be elongated.

If, then, the atmosphere of Jupiter was sufficiently thick to enable the satellites to be seen through it, two marked phenomena would occur: the satellite would be distorted into an ellipse,

and the occultation would be retarded. The effects of the first would be revealed by observation, and the effects of the second must imperatively reveal themselves in forming tables of the Jovian satellites. It may be regarded as certain that if the occultation of the satellites were sensibly retarded by the effects of an atmosphere, the constant difference between the duration of an occultation and of a transit of the same satellite could not have escaped attention. This last objection, it is true, applies solely to the case of a transparent atmosphere extending beyond the uppermost cloud bearing strata, which forms the apparent limb of the planet at which the satellites generally seem to disappear.

There is, however, still one more phenomenon which would occur during the occultation of a satellite by Jupiter, were this to be seen to take place through the Jovian atmosphere. It is well known that light is absorbed in traversing any gaseous medium, and thus decreased in intensity. It is, moreover, known that the amount of this absorption varies directly as the mass of gas which is traversed. The amount of light which is thus absorbed in passing through a given thickness of medium is comparatively considerable. Thus, in crossing the terrestrial atmosphere perpendicularly from the zenith, the sun loses nearly twenty per cent. of its light. Yet the amount of air which is passed through is only a few miles in thickness. The solar light reflected from the satellites of Jupiter during occultation will have to traverse considerable thicknesses of atmosphere, so that, through absorption, they will lose a very considerable amount of light. If, then, the sun loses one-fifth of its brightness in traversing the thirty miles of air which practically forms the terrestrial atmosphere, it is evident that when the light from the satellites of Jupiter have to pass through several hundred miles of the atmosphere of the planet, they must seem to undergo an enormous decrease in brightness. But were the satellite seen through a layer of atmosphere two thousand miles beneath the surface, it would have to pass through twenty-five thousand miles of atmosphere. Even, therefore, were the atmosphere very much rarer than the terrestrial atmosphere, the great majority of its light would be absorbed. There can be no question, therefore, that as the light from the satellite was compelled to pass through the atmosphere, its brightness would rapidly decrease.

Before proceeding further it will be advisable to ascertain the amount of refraction which a pencil of light would undergo in passing through the Jovian atmosphere. Taking the standard density of the air at the terrestrial surface for unity, let δ represent the density of the strata of air crossed by the pencil of light when nearest the centre of the planet. Then suppose α to

denote the power of refraction, and κ the specific gravity of the principal constituent of the atmosphere. Finally, let x denote the apparent geocentric refraction of the pencil of light. That is, if from the earth the angular distance of the source of light from the centre of Jupiter's disc were really $25''$, say, the effect of the refraction of the light would be to make it appear at a distance of $25'' + x$ from the centre of Jupiter.

Then approximately

$$x = 7'' \cdot 200 \cdot \alpha \cdot \frac{\delta}{\kappa} \frac{\text{Distance of the source of light from Jupiter}}{\text{Distance of the source of light from the Earth.}}$$

By means of this expression the refraction can be easily computed, and if, in accordance with probability, the atmosphere be supposed analagous to our own, α and κ can be put equal to unity, or, in other words, omitted.

Examine more closely what really has been seen to occur during the occultation of one of its satellites by the giant planet Jupiter. The descriptions given by Messrs. Ringwood and Todd, which have been already quoted (*Astronomical Register*, September, 1877, pp. 227, 228), are clear, and they are entirely confirmed by the earlier observations of Messrs. Hodgson and Wray.* The satellite slowly and steadily moves up to the visible limb of the planet, and commences to disappear there. It forms a sort of protuberance gradually diminishing in size until the satellite appears bisected, from one half having disappeared beyond the limb. Immediately after this the whole of the satellite can be detected as if it were projected on the planet. As the satellite moves forward, its whole disc remains visible, exactly as if it were commencing its transit across Jupiter, the only difference being that it is slightly fainter. Suddenly the satellite completely disappears with considerable rapidity, after having been apparently completely projected on the disc, as if it were in internal contact at transit.

The hypothesis which has been advanced to account for this phenomenon is simplicity itself, and is therefore admirably well adapted for popular exposition. It assumes that the planet Jupiter possesses an atmosphere of very considerable depth, and that this atmosphere is filled with minute white clouds. It supposes that what is seen is the upper surface of this cloud-bearing atmosphere, and assumes that this gaseous envelope extends for

* I have only lately come across this observation of Wray's. Using a telescope of exactly the same aperture as the Adelaide observers, he saw the same phenomenon as described by them. A full description will be found in the *Astronomical Register*, vol. 1, p. 95.

at least three thousand miles beneath the visible uppermost strata of clouds. It further supposes that though this cloud-bearing atmosphere reflects nearly as much light as the rest of the planet, it is at the same time sufficiently transparent for a bright object to be seen through it. The phenomenon seen by Messrs. Todd and Ringwood is then explained by supposing that the satellite can be seen through this semi-transparent atmosphere which borders the planet.

It has been already shown that this hypothesis, by assuming that the planet Jupiter possesses an atmosphere extending for several thousand miles beneath the uppermost cloud-bearing strata, is invalidated by assuming a condition dynamically impossible. For it has been shown that such a great depth is not possible, because long before such a depth were reached, the immense pressure would crush the atmosphere into a form, when it would be to all intents fluid, not gaseous. It is, however, to be shown, that even were this not so, this hypothesis would still fail to account for the observed appearance.

Assume that it were possible for Jupiter to possess an atmosphere extending for three thousand miles beneath the uppermost cloud-bearing strata. What effect would this atmosphere have on the occultation of a satellite?

First examine how the cloud strata must be constituted. It is known that the border of Jupiter, although not so bright as the central portions, is not very much fainter. The central regions are not so bright as the brightest satellite, whilst the border is brighter than the faintest satellite. It follows that this deep atmosphere of Jupiter cannot be filled with these minute clouds, for the border of the planet, where the solar light crosses only a few hundred miles of atmosphere, is almost as bright as the portion a little within where the solar rays must traverse thirty thousand miles of atmosphere. Therefore, were the clouds uniformly distributed, if a few hundred miles of cloud strata reflected so much light at the place where so many thousand miles had to be traversed they would reflect all. It would therefore stop all the light from the satellite, just as it stopped all the light from the sun. On the other hand, if three thousands of miles of atmosphere failed to reflect all the solar light, but were so transparent that less than half were arrested, then it would indeed be possible to see the satellite through it; but, at the same time, at the border of the planet, where the solar rays would pass through only a few hundred miles of atmosphere, scarcely any of the solar light could be reflected, and it would not form the visible border of the planet as it is by the hypothesis supposed to do.

The clouds, therefore, cannot be uniformly distributed. They must be supposed distributed in an artificial manner through a comparatively thin strata of the atmosphere. In this manner this part of the difficulty in their distribution is got over at a sacrifice of probability, for any hypothesis which assumes a clear atmosphere nearly three thousand miles high, surmounted by a permanent strata of clouds of small thickness at the best is but conceivable. It is further to be remarked that either these clouds are the thinnest of the thinnest, so filmy and transparent that a cubic foot of the faintest terrestrial mist would supply a cubic mile of them, or else if denser must be very scarcely packed. A few dozen feet of any terrestrial clouds completely hides the sun, whereas by the hypotheses a hundred miles of these Jovian clouds does not go far in dimming the light of Jupiter's satellites. The real clouds of Jupiter must be, therefore, very different to these, and according to the hypotheses being considered must lie three thousand miles beneath, with a clear strata of cloudless atmosphere between the two. The origin of these loftier clouds must be something of which we have no conception. But these things are only difficulties, by the bountiful sacrifice of probability each could be overcome by piling improbability on improbability, assumption on assumption. Yet, as will be seen, even this is not enough.

It has been shown that owing to the effects of refraction, if during its occultation a satellite was seen through the atmosphere of Jupiter, it would appear distorted into an ellipse. Now, nothing of this kind has been observed, but the satellite when seen as it was projected on Jupiter presents its normal circular disc. It is, therefore, certain that the light from the satellites undergo little, if any, refraction before reaching the earth. If the refraction at two to three thousand miles beneath the surface were greater than that at the surface by little more than one-fourth of the apparent diameter of the satellite (seen from Jupiter), the satellite, as seen from the earth, would appear to be flattened into such a narrow ellipse that the distortion could not possibly be overlooked. The satellite would in fact appear with an elliptical disc, of which the greater diameter would be nearly half as long again as the minor. But no such distortion having been seen, it is quite certain that the difference in refraction undergone by the rays at the visible surface of the cloud strata of the atmosphere, and at a point two or three thousand miles beneath, cannot amount to much more than a fifth of the visible diameter of the satellite.

What condition does this fact indicate? To ascertain this, return to the formula which has been given for calculating the

amount of the refraction which a pencil of light would undergo in traversing the atmosphere of Jupiter. Suppose that the IV. satellite is being occulted. Then

$$x = 43'' \frac{\alpha}{\kappa} \delta.$$

The diameter of the IV. satellite is about 2,500 miles. The condition which has been found above is equivalent to the following:—That if, at a depth of $\frac{1}{5} \times 2,500$, the value of x is greater than it is at the surface by one-fifth of a second of arc, the satellite will appear sensibly distorted. As the satellites do not appear distorted, this limitation must not be transgressed. If, therefore, δ_1 be the density at the upper cloud-bearing strata, and δ_2 the density 2,000 miles beneath, the greatest value of the last must be

$$\delta_2 = \delta_1 + \frac{1}{215} \cdot \frac{\kappa}{\alpha}$$

If in accordance with analogy the atmosphere of Jupiter be supposed to be composed of gases similar to those of our own, the factor α must be supposed not less than unity. The presence of aqueous vapour in the atmosphere, and it has been shown to exist, would indicate that refractive power to be considerably greater than unity. Indeed, with one solitary exception, the gases constituting terrestrial air possess a much smaller power of refraction than any other known gas, however much lighter these may be. This single exception is hydrogen, the lightest of all known bodies, fourteen times lighter than air. Yet even hydrogen possesses a refractive power only a little more than one-fourth less than the refractive power of air. It cannot, therefore, be supposed that the refractive power of the atmosphere of Jupiter is much smaller than that of our own. Indeed, even if there be invented for its principal constituent some hypothetical new element of much less specific gravity than air, the example of hydrogen shows that its refractive power cannot be legitimately supposed much less than that of air. If then the value of α , the refractive power of the atmosphere of Jupiter, be taken as only half, a most excessively small and, therefore, favourable value will have been assumed. It is in fact equivalent to creating a new hypothetical element with properties expressly favourable for this hypothesis which is being considered.

It has been already stated that the uppermost cloud-bearing strata of the atmosphere of Jupiter must possess a density of at least one hundredth of the standard density of air, or it would be incapable of maintaining a permanent layer of cloud; and by the hypothesis being examined, it does maintain this layer of

cloud. Therefore $\delta_1 = \frac{1}{100}$. Therefore substituting these values of α and δ , in the expression which has been found for δ_2 and

$$\delta_2 \frac{1}{100} + \frac{\kappa}{215} = (1 + \frac{15}{7} \kappa) \times \frac{1}{100}$$

Now κ is the specific gravity of the wonderful new body, and must be much less than unity, because it has been proved that were the atmosphere composed of gases as heavy as air a depth of two thousand miles is impossible, on the ground that the air would be crushed out of its gaseous condition by the immense superincumbent pressure. Suppose, however, $\kappa = \frac{1}{10}$, and thus approximate to what the real value must be to fulfil the required condition. Then

$$\delta_2 = \frac{17}{14}$$

That is to say, that a depth of two thousand miles beneath the surface of the cloud-bearing strata of the atmosphere, the density must be only $\frac{3}{14}$ greater. By hypothesis this great depth of air is cloudless, as it has been shown in the preceding pages, it must, therefore, be nearly uniform in temperature and cannot be heated. Otherwise there would be heat currents, clouds must condense, and either the whole mass be filled with clouds or be surmounted by a thick cloud layer from the condensation of the substances vaporised by the high temperature. The hypothesis which is being examined assumes all these to be absent, for it supposes a mere filmy layer of clouds and a clear under strata.

Under these conditions of a uniformly low temperature it has been shown that, were the Jovian atmosphere as dense as air, it would increase in density tenfold for every five miles. By the condition found the density of the hypothetical new body is so slight that it only increases in density by $\frac{3}{14}$ ths for a depth of 2,000 miles. It follows, therefore, that its density must be less than one-hundredth of the density of air, for otherwise it would increase in density tenfold for every 500 miles. It has been shown that if of less density than one-hundredth of the density of air* it would not support a cloud-bearing strata.

Although, therefore, extremely favourable conditions have been assumed for the hypothesis, it is found to be inconsistent with the laws of dynamics. What condition must therefore be assumed to render this hypothesis possible? Continuing the approximation, the following results will be found: To render it possible for the satellites of Jupiter to be seen during their occultation through

* Throughout the word density has been used in the true sense of mass per unit of volume under the standard conditions.

an atmosphere to that planet without being sensibly distorted by refraction, it must be assumed that the gas composing the atmosphere is some hypothetical new element, 5,000 times lighter than atmospheric air, and with a power of refraction not one-tenth as great, whilst the upper cloud-bearing strata which forms the visible boundary of Jupiter must be supposed to consist of a thin belt of clouds, suspended in a medium whose density is not one fifty-thousandth of the density of the terrestrial atmosphere. An hypothesis which depends on such utterly improbable assumptions as these is worthless. That permanent layers of clouds of any constitution whatever could exist in so rare a medium is inconceivable.

Even, however, this is not sufficient to bring the hypothesis in question within agreement with observation. The atmosphere would not stop short at the supposed upper cloud-bearing strata. Just as the hypothesis supposes the density to increase so exceedingly slowly as the depth beneath the clouds increases, so the density must decrease very slowly as the distance above the clouds increases. At a distance of 10 to 15 seconds of arc from the planet this hypothetical atmosphere would be still nearly one-tenth as dense as at the border of the planet. This introduces a new difficulty which can only be removed by means inconsistent with that enabling the last difficulty to be overcome. For stars have been not unfrequently seen within this distance of Jupiter, and the planet slowly moves past them without any alteration in the position of the stars having been noticed. The refraction of the light from a star by the atmosphere of Jupiter is a very different thing to the refraction of the light of the satellites. When seen from the earth the apparent change of place is very much greater, as a reference to the formula will show. Although, therefore, at a distance of 10 seconds of arc from the edge of the planet a satellite would have its place altered by refraction by only about one-twentieth of a second of arc, a star would have its place altered by nearly 10 seconds of arc. Therefore, any star which approaches close to Jupiter would appear to move in an extraordinary semi-circle around the planet.* Nor would this be all, for the satellites would undergo a sufficiently marked retardation at occultation, and a sufficiently great acceleration at reappearance for this fact to be discovered in forming tables of the satellites. Sufficient, however, has been said to show that it is impossible that Jupiter could possess an atmosphere extending 3,000 miles beneath its upper cloud-bearing strata, and sufficiently

* An occultation of a star by Jupiter would be impossible under these conditions, and yet one or two have been seen.

transparent to show the satellites of Jupiter through it without any distortion in their form. The interpretation which has been put on these observations of Messrs. Todd and Ringwood, which was that the satellite could be seen through its atmosphere, and that consequently the atmosphere must be of great depth, depends on a condition of this atmosphere being assumed which it could not possibly possess. The interpretation, therefore, is invalid.

This subject has been considered at length because it has been claimed as a proof positive of the correctness of the theory of the constitution of Jupiter, advanced and elaborated by Mr. R. A. Proctor. Mr. Proctor's theory of the physical condition of the great planet to our solar system is a most valuable contribution to our knowledge of physical astronomy, and its probability strengthened by many arguments of great force. It would, therefore, be a decided misfortune if a theory strong enough to stand by its own weight should be weakened by an untenable hypothesis being advanced to the dignity of one of its main supports. It was therefore thought well to thoroughly examine the hypothesis at the base of those charmingly simple methods of explaining the appearance and physical condition of Jupiter by assuming it to possess an immense extent of atmosphere. It may be regarded as certain that it is physically impossible for Jupiter to possess an atmosphere of great depth, unless the temperature of the planet be supposed to be many million times hotter than a white heat, or to have recourse to some imaginary body for the constitution of this atmosphere. And supposing one of these utterly improbable assumptions to be made, the physical condition of the planet will be found to be inconsistent with the observed phenomena.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE ROYAL ASTRONOMICAL SOCIETY.

Sir,—It appears that the Fellows of the Astronomical Society are to be called upon at the next General Meeting to vote upon sundry suggested changes of our Bye-laws—either as proposed by the Council (probably by a very narrow majority) or as amended (with the obvious wish to render the changes as innocuous as possible) by Lord Lindsay. It is much to be

desired in the interests of all who respect the dignity and independence of the Society that Lord Lindsay's amendments should prevail against the original motion, though for my own part I should be glad if afterwards even the amended changes, when put as a substantive motion, should be rejected, so that no trace of the divisions which have so long disgraced and injured this Society should be left permanently on record.

I suppose the Society will at least be informed, on February 8th, *why* our laws, which have so long worked smoothly, require now to be altered. If the truth is told, the reason will be found to be simply this, that a section of the Council, once in the minority but now slightly in the majority, bold that the Council's office is to *govern*, not to *serve* the Society. The endowment question, though certainly the perturbing cause, was not and is not the real moving cause of our differences. Even among those who think that the Society must be hectoring into submission to the "governing body," there are but few, I feel sure, who are not honestly and staunchly opposed to the set of jobbers who have tried to trade upon the Society's influence. But the section which regards the body of the Society as "outside Fellows" has always been powerful in the Council. Gradually, partly through resignations, but chiefly through its own concerted action, this section has removed the stauncher upholders of the interests of the body corporate. But, becoming impatient, it tried last year to strike two of the opposition at once. It failed to effect its purpose. *Hinc ille lacrymæ.* The Society will have to decide at the coming meeting whether the failure of a very narrow majority of the *governing body* to remove two of the most faithful *servants* the Society has ever had is to be regarded as just and sufficient reason for modifying our Bye-laws. I sincerely hope that the Society will worthily assert its dignity and independence.

Yours faithfully,

RICHARD A. PROCTOR.

P.S.—If, as is possible, an alternative Council-list should be named by those who object to the pedagogic tone of a portion of the present Council, let it be remembered that unanimity among the supporters of the opposition list is essential to its success. This unanimity must extend to the names cancelled as well as names substituted, or the opposition will inevitably fail.

NAMES ON THE MAP OF THE MOON.

Sir,—The completion of your "Explanation of names on map of the Moon" in the December number of the *Astronomical Register*, suggests to me to send you a few notes which I have made with reference to some of those names concerning which there is "Explanation wanted."

First then, "Alexander" may have been (1) The Great; (2) A. Trallianus, who wrote, in Greek, on philosophy in the fourth century; or (3), and most probably, the Ephesian poet (mentioned by Strabo) who wrote on astronomy and geography.

"Ariadaeus," pro Aridaeus, the friend of Cyrus the younger.

"Büsching," geographer and philosopher, died in Berlin, May, 1793.

"Eichstädt," a well known Bavarian town.

"Fra Mauro," was Francis Maurolico (or Maurolico), Abbot of Messina, who died July 21, 1575. He was famous as a writer on geometry, optics, and arithmetic.

"Hansen," probably a misprint for Hansen.

"Hippalus," the first man who sailed on the open sea from Arabia to India.

"Messala," the Roman orator and philologist, who died A.D. 11.

"Moretus," was the son-in-law of Plantin, who founded the first printing press in the Netherlands. He was the friend of all the philosophers of his day, and printed many works in science, including astronomy. The copper plates of one of his astronomical books are still extant. His descendants have preserved his house in its original condition in the city of Antwerp; and the municipality are about to purchase it, with all its contained treasures, MSS., pictures by Van Dyck, &c., for 1,200,000 francs.*

"Rocca," is the Italian for a castle or keep on a rock. I do not remember the aspect of the crater so-called, but would suggest that something in its configuration may have given the idea for its name.

"Seguer," a Jesuit, famous for his eloquence, who died in Rome in 1694.

"Sömmering," the well known anatomist and microscopist, inventor of the little steel mirror, often used to draw with at the microscope instead of the camera lucida.

I am, sir, obediently yours,

Forest Lodge, Maresfield,
Uckfield: December 10, 1877.

WILLIAM NOBLE.

JUPITER'S ATMOSPHERE.

Sir,—Mr. Proctor's curious idea that I have strangely misapprehended the formulæ, by which I prove that the depth of the atmosphere of Jupiter cannot exceed a certain number of miles, must have arisen from his having omitted to examine the formulæ criticised, for otherwise he could hardly have so completely failed to understand them.

Mr. Proctor seems to labour under the idea that these formulæ were devised by Ivory, and form merely an approximate solution of the problem of the refraction of a pencil of light through the terrestrial atmosphere, which cannot be applied legitimately to any other problem. In this Mr. Proctor does "strangely misapprehend" the formulæ.

Based on a complete solution of the problem of the refraction of a pencil of light through a spherical atmospheric envelope of any density, the foundation of the formulæ is independent of any assumption. The law of decrease of density, pressure, gravity, &c., on which they depend, are those whose truth has been established by experiment as well as by dynamical theory of gases. They are completely true for an atmosphere to any planet whatever, be its density what it may. The foundation of the formulæ are, therefore, entirely independent of any assumption of merely partial truth, and which would limit their application to the particular case of the earth.

In fact, although Mr. Proctor takes exception to my applying them to the case of the moon, Bessel, one of the greatest mathematicians who ever lived, and a high authority on this very subject, has done exactly the same, and by means of a particular case of the same formulæ (independently derived), calculated the maximum density and horizontal refraction of any lunar atmosphere. Mr. David Trowbridge, an American

* I am indebted to my friend, the Baron von Erthou, for these details concerning Moretus.

mathematician, likewise has independently derived and applied a second particular case of the same formulæ to the case of the greatest depth of the atmosphere of Jupiter, and obtained results entirely entirely confirming my own.

Mr. Proctor's idea, that these formulæ are based on empirical laws and which are only true of the condition of the atmosphere of the earth, is entirely a mistaken idea; there is nothing empirical about them.

It is possible that Mr. Proctor may have misconstrued the form in which the law of decrease of density is made to depend on the law of decrease of temperature. It is true that this is given in a form similar to Ivory's equations. But if so, he must have overlooked the fact that these really possess a general form. As they stand, by giving suitable values to the different constants, the formulæ represent not merely Ivory's equations, as Mr. Proctor supposes, but likewise Kramp's, Laplace's, Bessel's, Plana's, Lubbock's, &c.

When the formulæ have to be applied, it is true that the results obtained will to some extent depend on the law of decrease of temperature in the atmosphere, and this it is true is to some extent uncertain. But whatever law of decrease of temperature is assumed to be true, the form of solution will be unaltered, and the formulæ have been purposely left so that any law may be assumed at pleasure, and the proper result obtained. The formulæ give the two extremes, between which must be the true rate of decrease of temperature, and the results must decrease from one of them to the other, for a maximum or minimum cannot be between them. The limiting condition is therefore fixed, between which the true state of the atmosphere must lie, and these limits are narrow. Whatever, therefore, may be the true rate of decrease of temperature in the atmosphere of a planet, it cannot materially alter the result which would be obtained by the assumption of any other rate of decrease. And it must not be overlooked, that science furnishes a number of means of approximating to the true rate of decrease of temperature. Save in this point, the solution of the problem is definitely obtained in a rigid form.

It is unnecessary for me to add more, the fanciful illustration with which Mr. Proctor illustrates my supposed error has no real bearing on the subject, unless it is meant to indicate that Mr. Proctor has never thought fit to read Bessel's investigation on the refraction of light.

E. NEISON.

THE TRANSIT OF VENUS AND PHOTO-HELIOGRAPHY.

The calculations necessary for the reduction of the British observations of the late transit of Venus being complete, the Astronomer-Royal announced, at the meeting of the Royal Astronomical Society, on November the 9th last, that the solar parallax thus reduced was $8''.754$, corresponding to a mean distance of the sun's centre of 93,375,000 miles. Grave doubts have since been thrown upon the accuracy of this result in articles which have appeared in the *Times*, the *Spectator*, and the *Echo*; and it is undoubtedly a most significant fact that the two groups into which the British observations can be divided, that of ingress and that of egress, afford results differing by more than a million of miles in the sun's distance.

It is not, however, with this aspect of the question with which we propose here to deal: it is with the issue of the much vaunted photo-

heliographic method of determination, which certainly deserves much more serious notice, on many accounts, than it has so far received. Every Fellow of the Royal Astronomical Society who attends its evening meetings, must remember how, from time to time, the adoption of this method has been insisted upon by Mr. De La Rue; and how suggestions as to the untrustworthiness (in a metrical point of view) of photographs taken in a secondary focus, have been pooh-poohed by him and his little following. We need only refer to our own report of the discussion which took place at the meeting on May 14, 1875, (*Astronomical Register*, Vol. XIII., pp. 136 and 137) to show how thoroughly Mr. De La Rue accepted the entire responsibility of the photographic arrangements, and with what perfect disdain he treated all those who ventured to suggest that his method was faulty in theory, and would probably break down in practice. What, however, has the nation received in return for the expenditure incurred at his instance?

Just this. These most valuable photographs have now been discussed and measured with all the accuracy of which they are susceptible; and afford a parallax (or series of parallaxes), varying between $8''.2$ and a minns quantity!!! In other words, according to this most trustworthy method of determination, we may be 99,680,990 miles from the sun, or we must have been on one side of him while Venus was on the other! Surely this is a lesson which should not be lost upon the Fellows of the Society when plausible appeals are made to them to countenance and encourage photo-heliography for any scientific purpose. *Ars probat artificem*—"By their fruits ye shall know them;" and we can only hope that this utter fiasco will not be lost upon those who in a merely common-sense way judge dogmatic predictions by their ultimate accomplishment or non-fulfilment.

Books received.—*Annuaire de l'Observatoire Royal de Bruxelles*. Bruxelles: F. Hayez. 1877.—*Transactions of the Royal Irish Academy*. Vol. XXVI. Dublin: 1877.—*Papers on Physical Science*. By Humphrey Lloyd, D.D., &c., &c. London: Longmans, Green & Co. 1877.—*Treatise on the Cycloid and Cycloidal Curves*. By R. A. Proctor, B.A., &c., &c. London: Longmans, Green & Co. 1878.—*The Library Table*. New York. 1877.—*The Universe of Stars*. By R. A. Proctor. 2nd Edition. London: Longmans, Green & Co. 1878.—*Other Worlds than Ours*. By R. A. Proctor. 4th Edition. London: Longmans, Green & Co. 1878.—*Results of the Astronomical Observations made at the Royal Observatory, Greenwich*. 1875.—*Dun Echt Observatory Publications*. Vol. II. Mauritius Expedition. 1874. Division I. Dun Echt, Aberdeen. 1877.—*Transits of Venus*. By R. A. Proctor, B.A., &c. 3rd Edition. London: Longmans, Green & Co. 1878.—*A New Star Atlas*. By R. A. Proctor, B.A., F.R.A.S. 4th Edition. London: Longmans, Green & Co. 1878.—*Southern Stellar Objects*. By J. E. Gore, M.R.I.A., &c. Lodi and Mission Press. 1877.

Errata January No., p. 20, line 2, for *which* read *while*, lines 4 and 5, for *applying an atmosphere* read *applying to an atmosphere*, line 7, for *refractive motion* read *refractive action*, page 23, line 10, after "disc of" supply $0''.1$, and at line 11, after "disc of" supply $0''.05$, line 15, for *two radii*, read 2.6 , line 16, for *six radii* read 6.8 .

ASTRONOMICAL OCCURRENCES FOR FEBRUARY, 1878.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Fri	1	20 17	● New Moon Eclipse of the Sun, invisible at Greenwich			Procyon — 10 44.9
Sat	2		Sidereal Time at Mean Noon, 20h. 50m. 20.58s.			10 41.0
Sun	3	23	Conjunction of Moon and Venus 4° 54' N.	1st Sh. I. 1st Tr. I.	18 40 19 11	10 37.1
Mon	4		Sun's Meridian Passage 14m. 12.07s. after Mean Time	1st Oc. R.	18 39	10 33.2
Tues	5	0	Conjunction of Moon and Saturn, 4° 52' S.			10 29.2
Wed	6	6	Disappearance of Saturn's Ring Conjunction of Jupiter and Mercury 0° 23' N.	2nd Sh. I.	18 57	10 25.3
Thur	7			3rd Oc. R.	19 14	10 21.4
Fri	8	22	Conjunction of Moon and Mars 4° 32' S.			10 17.4
Sat	9					10 13.5
Sun	10	1 16	☾ Moon's First Quarter			
		8 57	Occultation of 7 Tauri (6)			
		13 48	Occultation of 13 Tauri (5)			10 9.6
		14	Near approach of 16 Tauri (5½) 14			
		14 13	Occultation of 20 Tauri (5)			
Mon	11					10 5.6
Tues	12	11 14	Occultation of B.A.C. 1746 (6½)			10 1.7
		11 36	Reappearance of ditto			
Wed	13					9 57.8
Thur	14		Illuminated portion of disc of Venus=0.017			
		16 30	Illuminated portion of disc of Mars=0.892			
		17 3	Occultation of 84 Geminorum (6½) Reappearance of ditto Saturn's Ring : Major axis=37".74 Minor axis= 0".54			9 53.8

DATE.		Principal Occurrences.	Jupiter's Satellites.	Meridian Passage.
		h. m.	h. m. s.	h. m.
Fri	15	6 10	Near approach of η Cancri (6)	Procyon
		7 43	Near approach of 35 Cancri (6½)	—
		9 35	Occultation of α Cancri (6)	
		10 43	Reappearance of ditto	
		9 52	Occultation of B.A.C. 2925 (6½)	9 49.9
		10 48	Reappearance of ditto	
		9 57	Occultation of 42 Cancri (6½)	
		10 16	Reappearance of ditto	
	17	Opposition of Uranus to the Sun		
Sat	16	23 16	O Full Moon	
		7 50	Occultation of 7 Leonis (6½)	9 45.9
		8 25	Reappearance of ditto	
		Eclipse of the Moon invisible at Greenwich		
Sun	17	9 22	Occultation of 48 Leonis (6)	9 42.0
		10 16	Reappearance of ditto	
Mon	18	13 40	Near approach of ν Leonis (4½)	9 38.1
Tues	19	Sidereal Time at Mean Noon 21h. 57m. 12.02s.		9 34.2
Wed	20	17	Inferior conjunction of Venus and the Sun	9 30.2
Thur	21	Sun's Meridian Passage 13m. 50.30s. after Mean Noon		9 26.3
Fri	22			9 22.4
Sat	23	15 12	ϵ Moon's Last Quarter	9 18.4
Sun	24			9 14.5
Mon	25	16 20	Occultation reappearance of B.A.C. 6191 (6½)	
		18 3	Occultation of B.A.C. 6220 (6½)	3rd Tr. E. 18 25 9 10.6
		19 9	Reappearance of ditto	
Tues	26			2nd Oc. R. 13 12 9 6.6
Wed	27	17	Conjunction of Moon and Jupiter $2^{\circ} 7' N.$	9 2.7
Thur	28			8 58.8
MAR				
Fri	1	Reappearance of Saturn's Ring		8 54.8
Sat	2	0	Conjunction of Moon and Venus $8^{\circ} 47' N.$	8 50.9

THE PLANETS FOR FEBRUARY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	19 17 48	S. 21 19½	6".9	22 27.9
	9th	19 56 50	S. 21 5½	6".1	22 35.4
	17th	20 42 40	S. 19 35	5".6	22 49.6
	25th	21 31 58	S. 16 41½	5".2	23 7.3
Venus ...	1st	22 40 1	S. 2 40	51".6	1 53.4
	9th	22 31 5	S. 1 41	57".0	1 13.1
	17th	22 14 24	S. 1 57½	60".1	0 25.0
	25th	21 53 39	S. 3 32½	59".2	23 28.9
Mars ...	1st	1 54 58	N. 12 35½	8".0	5 7.9
	9th	2 14 2	N. 14 22½	7".6	4 55.5
	17th	2 33 30	N. 16 3½	7".2	4 44.9
	25th	2 53 24	N. 17 38	6".9	4 31.8
Uranus ...	2nd	10 1 49	N. 12 54½	4".2	13 9.5
	18th	9 59 9	N. 13 9	4".2	12 3.9
Neptune ...	2nd	2 12 44	N. 11 29	...	5 21.7
	18th	2 13 39	N. 11 34½	...	4 19.7

Mercury may be observed for about an hour and a quarter before sunrise at the beginning of the month, the interval decreasing to about a quarter of an hour on the 28th.

Venus is visible for three hours after sunset at the beginning of the month, the interval rapidly decreasing. She may be observed about an hour before sunrise on the last day.

Mars sets shortly after midnight throughout the month.

Uranus will be at opposition to the sun on the 17th.

SATELLITES OF URANUS.

Major and minor semi-axes a and b of the apparent ellipses described by the satellites, and position-angles p° of the major axes:

1878.	p°	Ariel.		Umbriel.		Titania.		Oberon.	
		a	b	a	b	a	b	a	b
Feb. 4.	8.24	15.22	4.61	21.21	6.43	34.79	10.54	46.52	14.09
14.	8.35	15.24	4.73	21.24	6.59	34.83	10.80	46.58	14.44
24.	8.47	15.24	4.83	21.23	6.73	34.82	11.05	46.56	14.77

The satellites will be at their greatest elongations N. and S., and at their sup. and inf. conjunctions with the planet, about the following hours, G.M.T.:

Ariel.		Umbriel.		Titania.		Oberon.	
1878.	b.	h.		h.		h.	
Feb. 2.	2 S.	Feb. 2.	7 N.	Feb. 2.	21 N.	Feb. 3.	10 S.
3.	8 N.	4.	9 S.	5.	1 inf.	6.	18 sup.
4.	15 S.	6.	11 N.	7.	5 S.	10.	3 N.
5.	21 N.	8.	13 S.	9.	9 sup.	13.	12 inf.

7. 3 S.	10. 14 N.	11. 14 N.	16. 21 S.
8. 9 N.	12. 16 S.	13. 18 inf.	20. 6 sup.
9. 16 S.	14. 18 N.	15. 22 S.	23. 14 N.
10. 22 N.	16. 20 S.	18. 2 sup.	26. 23 inf.
12. 4 S.	18. 21 N.	20. 7 N.	Mar. 2. 8 S.
13. 10 N.	20. 23 S.	22. 11 inf.	5. 17 sup.
14. 17 S.	23. 1 N.	24. 15 S.	
15. 23 N.	25. 2 S.	26. 19 sup.	
17. 5 S.	27. 4 N.	Mar. 1. 0 N.	
18. 11 N.	Mar. 1. 6 S.	3. 4 inf.	
19. 18 S.	3. 8 N.		
21. 0 N.			
22. 6 S.			
23. 12 N.			
24. 19 S.			
26. 1 N.			
27. 7 S.			
28. 13 N.			
Mar. 1. 20 S.			

Feb. 2.—Uranus passes in its retrograde course α Leonis in right-ascension, about 21° north.

A. M.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN FEBRUARY, 1878.

By W. R. BIRT, F.R.A.S., F.M.S.

THE NORTH-WEST ENVIRONS OF PLATO.—The recently formed "Selenographical Club," mentioned in the *Astronomical Register* for November, 1877, p. 302, being engaged in an examination of the objects north-west of Plato, it may be as well to specify a few of the most conspicuous. One of the most important of these is the small crater G of B. and M. and Neison, situated close to the west border of Plato, in N. lat. $81^\circ 42'$, and E. long. $6^\circ 47'$, near the middle peak δ on the border. A line drawn from this crater towards the north-west will conduct the observer's eye to the conspicuous—under an early illumination—crater H of B. and M. and Neison. At full it does not appear to be so conspicuous as it has in its neighbourhood some spots of about the same degree of brightness. The line from G to H forms the S.W. boundary of the area under consideration, or rather examination. Neison specifies three other prominent objects, ν , π and σ within the area; π is placed by Neison nearly midway between G and H; ν is eastward of π , and forms with it a scalene triangle, differing but little from an equilateral. These mountains γ and π are recorded by Neison as about 6,000 English feet high. σ is a bright mountain on the northern border of Plato; according to a photogram taken on June 18, 1875, at the Observatorio Nacional Cordoba, for which I am indebted to an unknown donor G., γ and σ form an equilateral triangle. Neison assigns a brightness of 5.5 to G and of 5 to π , ν and σ .

The area marked out for examination is bounded on the S.W. by a line joining H and G; on the S.E. by one joining G and σ ; on the N.E. by a line which joins σ and γ , and on the N.W. by that which joins γ and

H. The area thus enclosed contains several very interesting objects, observations of which will be very acceptable to the club.

FRACASTORIUS.—The Rev. W. J. B. Richards, of Bayswater, calls attention to the shadows of the western wall of this formation, ending as it does with a sharp needle point. The contour of the western wall as it gradually declines in height to a point where it meets the surface of the Mare Nectaris would—according as the shadow is drawn by the observer which indicates the outline of the border as far as the peak, the shadow of which is well marked—be a concave curve between the surface of the Mare and the peak, and that the surface on which the shadow fell is either *flat* or slightly rising. The following published observations have not been confirmed:—The lower level north of Fracastorius, Dennett's second ridge on the Mare Nectaris and his *immense* depression noticed last month.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1877.

Common, A.
Dallmeyer, J. H.
De La Rue, W.
Freeman, G. T.
Gould, Rev. R. J.
Huggins, Dr. W.
Little, W.
Normand, J. A.
Ranyard, A. C.
Slater, J.
Waldegrave, Hon. H. N.

To Jan., 1878.

Fry, Miss.

To March, 1878.

Elliott, R.
Lock, W.
Wright, Rev. W. H.

To May, 1878.

Griffith, J.

To June, 1878.

Down, E.
Noble, Captain.

To Dec., 1878.

Alleyne, Sir J.
Clermont, Lord.
Collingwood, E. J.
Brewin, T.

Ellis, W. M.
Erck, Dr. W.
Garnett, W.
Green, G. W.
Hunt, G.
Jefferies, J.
Johnson, Rev. S. J.
Key, Rev. H. C.
Lee, J.
Lamb, Dr. W.
Perry, Rev. S. J.
Prince, C. L.
Readhouse, Miss C.
Redpath, H. S.
Richards, Rev. W. J. B.
Smyth, Prof. C. Piazzi.
Warriner, H.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for the Editor are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The *Astronomical Register* is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, payable in advance, by postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, Angel Court, Throgmorton Street, E.C., not later than the 15th of the Month.

The Astronomical Register.

No. 183.

MARCH.

1878.

ROYAL ASTRONOMICAL SOCIETY.

Session 1877—78.

The Annual General Meeting was held at the Society's Rooms, at Burlington House, on the afternoon of Friday, February 8th, 1878. The meeting commenced at 3 o'clock.

William Huggins, Esq., D.C.L., LL.D., F.R.S., &c., *President*,
in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the last General Meeting were read and confirmed.

The following candidates for the Fellowship of the Society were balloted for and duly elected :

Henry Baumer, Esq., 27, Harley Street, W.

Daniel Thomas Evans, Esq., 10, Kent Gardens, Castle Hill Park, Ealing, and 5, Elm Court, Temple.

John E. Griffith, Esq., Bangor.

Henry Burdett Hederstedt, Esq., M.I.C.E., 72, Lancaster Gate, W., and Lucknow, India.

Frederic G. Landon, Esq., M.A., 8, The Circus, Greenwich, S.E.

John Ambrose Ridgway, Esq., Foundation School, Beverley.

Benjamin Templar, Esq., Birkdale, Southport.

Richard Wilding, Esq., Cobridge, near Stoke-upon-Trent.

Parts of the annual report were read. Eleven ordinary Fellows and five Associates of the Society have died in the course of the past year. After taking into account the resignations and elections, the total number of Fellows of the Society has decreased by three, while the number of the ordinary Fellows has increased by two.

VOL. XVI.

A sum of £500 has been received by the Treasurer of the Society in the course of the past year from Mr. C. Lambert, a late Fellow of the Society. This sum, together with the Carrington and Janson legacies, brings the amount received by the Society in legacies in the course of the last two years up to £2,580.

The Council have to regret that Mr. S. C. Whitbread, who has held the office of Treasurer of the Society for the last twenty years, has been compelled by ill health to resign his post.

Twelve minor planets have been discovered in the course of the year; five of them within the last fortnight. Most of the newly discovered planetoids are only known by numbers, but the following five have been christened respectively, (170) Maria or Myrrha, (171) Ophelia, (172) Baucis, (176) Idunna, and (178) Belisana.

Five new comets have been detected since the last anniversary meeting, all moving in what appear to be parabolic orbits; in addition to these M. D'Arrest's comet, of short period, has been observed on the occasion of its fifth return to perihelion since its discovery in 1851.

Mr. Glaisher read a part of the report referring to the discovery of the satellites of Mars. He had received a communication from Professor Asaph Hall, mentioning that a systematic search for the satellites of Mars was commenced early in August. At first Professor Hall's attention was directed to faint objects at some distance from the planet, but all these proving to be fixed stars, he began to examine the region close to the limb. This was done by keeping the planet just outside the field of view, and turning the eye-piece so as to pass completely round the planet. On the 11th of August a faint object was observed on the following side, a little north of the planet, but its position had scarcely been secured when a fog, rising from the Potomac river, stopped the work. Cloudy weather intervened for several days, till on the 16th of August the object was found again on the following side of the planet. The observations of that night showed that it was moving with Mars. On the 17th, while waiting and watching for the outer satellite, the inner one was discovered.

The observations made on the 17th and 18th put beyond doubt the character of these objects, and their discovery was publicly announced by Admiral Rogers on the 18th. The satellites continued to be observed with the Washington 26-inch refractor until October the 31st, and Professor Hall deduces from his observations a period of 30h. 18.5m. mean time for the outer satellite, and 7h. 39.5m. mean time for the inner satellite. Both

the satellites move nearly in the plane of the equator of Mars, and both orbits are probably very nearly circular.

Both satellites have, it is believed, been observed in England; the outer satellite by Messrs. Christie and Maunder, at Greenwich, by Professor Pritchard, at Oxford, Lord Lindsay and Dr. Copeland, at Dunecht, Dr. Wentworth Erck, Mr. A. A. Common, Mr. Cooper Key, and Mr. Brett. The inner satellite appears to have been observed by Mr. Maunder twice, at Oxford once, and (?) once by Mr. A. A. Common.

Amongst the other matters referred to in the annual report were Mr. Stone's tables for "Star Constants;" Mr. Birmingham's catalogue of 658 red and orange stars, published in the *Transactions of the Royal Irish Academy*; Professor Newcomb's researches on the motion of the moon; Mr. Hill's paper on the motion of the moon's perigee; the Astronomer-Royal's numerical lunar theory; Professor Adams's paper on the motion of the moon's node; the continuation by Dr. Todd, of Damoiseau's tables of Jupiter's satellites; Dr. Draper's discovery of oxygen in the sun; M. Janssen's solar photographs; the unsuccessful search for the intra-Mercurial planet Vulcan; Mr. Gill's expedition for observing the opposition of Mars; Mr. Green's drawings of Mars; drawings of the planet Jupiter; Rotation of Saturn; The new star in Cygnus; and The progress of meteoric astronomy.

Mr. De La Rue said: As there may be some doubt still existing as to the actuality of the discovery of oxygen in the sun, would it not be well to slightly alter the title of the paragraph referring to it, and call it the "announced discovery" of oxygen in the sun?

The President: It was understood that the paragraph was to be altered so as not to involve the Council in any responsibility.

The Astronomer-Royal: I think it proper to say on this point that, when I received the intelligence from Dr. Draper, I interrogated him closely as to whether the photographs of the two spectra which were to be compared were taken contemporaneously, and whether they were the real untouched photographs formed by the same optical instrument; and his answer satisfied me that so far as the evidence of the photographs goes, there is nothing left to doubt; but still I am aware that gentlemen better acquainted with the subject than myself are doubtful as to the inferences to be drawn from the collation of these two photographs, and I therefore think it may be judicious to express, not any doubt, but a want of positiveness in speaking of the matter.

Mr. Ranyard: I feel sure the Society would not like to throw any slur upon the discovery of Dr. Draper, and I would suggest that instead of using the words "announced discovery," as if we

did not believe in it, for there are many in England who do believe in it, and who believe it is one of the most important discoveries ever made in solar physics, that the meeting should adopt the words "Professor Draper's researches with reference to the existence of oxygen in the sun." (Hear, hear.)

The President then gave the usual address in support of the Council award of the medal, of which the following is the substance : Before the Society gives effect to the award of the medal to Baron Dembowski, they will perhaps permit me to place before them some of the considerations which induced the Council to bestow on Baron Dembowski this high mark of their consideration. This is perhaps the more necessary, as the work of the medallist has not been of that original or remarkable character which carries within itself an obvious justification of the highest honour which the Society can confer. In saying this, I do not wish to admit that such work as the Baron has done is not quite as worthy of honour and reward as those more definite additions to knowledge called discoveries. As in the case of a perfect building, men attach a relative value to different parts, and while admiring the external arches and pillars, are sometimes apt to forget that a deep foundation of accumulated measurements and data were as necessary to the structure, as a whole, as those parts which are more ornamental, and which are apt first to strike the eye. With untiring energy and zeal Baron Dembowski has, for 25 years, pursued that part of astronomy which he has selected as most completely within his power to accomplish, viz., the micrometrical measurement of the stars of the Dorpat Catalogue. Such steady work, long persevered in, has never failed to reap its reward. The goddess of Truth is only to be won with accuracy, energy, and perseverance, and these are the gifts which the medallist has employed in the service of science, and which has won for him, most rightly, the award of the medal. The Society would not think he spoke too strongly when he told them that the Baron worked with an instrument with the very moderate aperture of 5-inches, and unprovided either with clock motion or position circle, and yet he boldly and at once undertook the revision of the brighter stars of the Dorpat Catalogue, and made up for the deficiencies of his instrument by his own skill. He was so successful that his measures, taken under these unfavourable circumstances, had come to be acknowledged by all who had worked in the same field as worthy of the greatest confidence. His untiring perseverance and diligence in the pursuit of this one thing had enriched science with a long connected series of measures, extending, without a break, over a period of 25 years. In his (the President's) opinion, it was such quiet, unobtrusive

work which it was pre-eminently the duty of the council to seek out and honour. (Hear, hear.) Great discoveries brought their own reward in the general attention and recognition which they at once claim and obtain for those who make them; but those who laboured among the foundations of science in the accumulation of measurements for other men to build upon were often but insufficiently remembered and honoured. In conferring on Baron Dembowski the honour of which he was so abundantly worthy, the Council wished to bestow a mark of encouragement on the class of workers represented by him, who are content to pass their lives in quiet work, seeking no honours from the outside world. The mode of publication of the Baron's observations, though rendering them at once available to other workers, was unfavourable to a due appreciation of the magnitude of his work. If all his observations which were now scattered through 70 numbers of the *Astronomische Nachrichten* were to be collected in one volume the catalogue would not be unworthy to stand beside the most valuable and extensive catalogues of double stars which we possess. The President then alluded to some of the details of the publication of the measurements. The first publication described the instrument with which the measurements were made, viz., a telescope, 5-inches in aperture, 5-feet in focal length, unprovided with clock motion or position circle, which formed usually an essential part of a measuring apparatus. The moving of the circle by hand was often difficult and irksome, but he (the President) would hazard the suggestion that those very difficulties, by the skill and perseverance which they developed in order to overcome them, contributed to the high quality of the Baron's work. Such severe training in surmounting severe difficulties was favourable to the formation of a masterly power and accuracy, which was not always found among observers who possessed from the first the luxury of the best instrumental aid. Baron Dembowski took for the measure of distance the mean of 10 observations of the same day, and for the measure of position the mean of five measures of the same day. Subsequent to 1864 the Baron was in possession of a much more powerful instrument—a refractor of 7-inches aperture provided with clockwork, a filar micrometer, and a position circle. From the "introduction" to his "new series" of measures it appears that the Baron made it a rule to take an even number of measures with the circle alternately placed in opposite directions, so as to ensure accuracy. The President concluded by indicating the various observations of Baron Dembowski which had been published periodically down to the year 1876. He then handed the medal to Lord Lindsay, with the request that he would, in his capacity of Foreign Secretary,

convey the medal to Baron Dembowski, with the assurance of the Society's highest appreciation of the accuracy of his long continued work, and of an earnest wish that he may have health still to continue his labours for many years to come. (Cheera.)

Lord Lindsay: Sir, I have received a letter from Baron Dembowski, in which he says that nothing but serious illness would have prevented him from coming to England to receive this honour in person. He had sent a letter which I will beg leave to read to the Society.

Lord Lindsay then read the letter, which expressed Baron Dembowski's high sense of the honour which had been conferred upon him.

Mr. Esdaile then read the report of the Auditors.

Admiral Sir C. F. A. Shadwell: I will propose, with your permission, that the report of the Council and the Auditors' report, now read, be received and adopted, and that it be printed and circulated in the usual manner, together with the President's address.

Major-General J. C. Hannington: I beg to second that motion.

Dr. Pinches: I have only one little observation to make, and it is this:—I question the expediency of removing the list of missing instruments from the general list. It seems to me, although we have not succeeded in finding them, it by no means follows that they are lost, and must, therefore, be regarded as utterly irrecoverable. It is just possible they may turn up at some time, and, if we abandon them as derelicts, I fear we may, perhaps, abandon any sort of legal claim we may have upon them. The practice hitherto, I think, has been to give a list of them at the bottom of the list of other instruments. Perhaps it would be better to still continue that practice. The other point I would name has no reference to this. Mr. De La Rue, I think, doubts the actuality of the existence of oxygen in the sun——

Mr. De La Rue: I merely stated that there was a difference of opinion.

Dr. Pinches: I merely wanted to say that I doubted the existence of a sufficient amount of oxygen in this room. (Laughter.)

The President: With reference to the missing instruments, I think Mr. Ranyard will, perhaps, state the reasons for omitting the list from the report.

Mr. Ranyard: For several years past the list of missing instruments has been printed over and over again. The description of one of them—"A thermometer"—is so vague that it would be difficult to trace it. There are only three other instruments. The first one—the Universal quadrant of Abraham

Sharp, was first mentioned in the report of the Council in 1847. There is no record of the instrument having been lent, and no information with regard to it can be obtained. This is, of course, a serious loss, as any instrument made by Abraham Sharp must be looked upon as of historical value. The next missing instrument is the Reade transit, about which no information can be obtained. It was first mentioned in the report of 1870, and I have been unable to obtain any information with respect to it. The third one is a small Sheepshanks' instrument, No. 6, lent to the Rev. R. J. Cape, in 1858. There is no entry of its return. Mr. Cape died in 1872, and his instruments were sold by auction, and all efforts to trace the instruments have been unsuccessful. Mr. Dunkin wrote more than one letter to the executors and to members of his family on the subject, and everything in fact has been done. It was only a small telescope, two inches in diameter, and I think it is hardly worth while to go on reprinting it in the list. We shall always have a record of the missing instruments in the old annual reports if we wish to turn to them.

The President put the motion for the adoption of the report, which was carried unanimously.

The President: You will recollect that to this meeting was adjourned the consideration of the amendments to the Bye-laws, which were brought before a previous meeting. I have to state with reference to them, that the Council do not intend to propose any amendments on the present occasion. Therefore, unless any Fellows wish to make any remarks on the matter, the subject may at once drop, and we can proceed to the next business. (Hear, hear.) Perhaps, on the whole, it would be the better course, and I hope, therefore, we may at once be able to proceed to other business.

The President: There is another small matter which we will take now. Two officers of the Society and a few Fellows have never yet been formally admitted. I will now call upon them to come forward.

The following Fellows were then formally admitted: Mr. Ran- yard, Mr. Marth, Lord Lindsay, Mr. Proctor, Mr. Thomas D. Brewin, Mr. Alexander Davidson, Mr. J. Kennedy Esdaile, Mr. T. H. E. C. Espin, Mr. Richard Inwards, Mr. William Ladd, Mr. G. Staley Mosse, and Mr. John James Mellor.

The President: Our next business is to proceed to the election of officers and Council. I have first to appoint the scrutineers, and I will appoint Admiral Sir Erasmus Ommanney, Mr. Crossley and Mr. Talmage.

Sir E. Beckett: I understood, Sir, you were going to ask the meeting to come to a conclusion on a certain question, but perhaps

you would prefer that the question should be put in the form of a motion by someone else.

The President: I would rather it came from someone else.

Sir E. Beckett: The question arose at last meeting, at which it will be remembered that an opposition list was circulated. I am not now going to say anything about the names on the opposition list, but this question will arise: The names have different offices attached to them; for instance, some are proposed as Vice-Presidents, some as Secretaries and Treasurers, and others as members of Council. Now, it is necessary to observe that the word "Council" has an ambiguous meaning. The "Council" in the Bye-laws, as far as I remember, means the governing body generally, including President, Vice-Presidents, Secretaries, Treasurer, and what are called Ordinary members of Council. Now, on some occasions, I understand that gentlemen have been proposed for a particular office and have not succeeded in getting the requisite majority for that office, and consequently they were out altogether. It appears to me, and I believe that I may say to a good many others also, that such a method of reckoning the votes is a mistake, and that the intention was that the votes should be for the governing body as a whole, so that first of all you elect a certain number of persons as members of the governing body, and then if you think fit you put a sort of extra mark against the name of anyone you wish to fill an office. And therefore, though you may fail in getting your man elected to the particular office, it ought not to follow that he is not to be a member of the governing body. Therefore it is very desirable that the meeting, as the matter is not clearly explained in the Bye-laws, should come to a conclusion in one way or the other. As far as my own wish goes, I am quite indifferent, but I have an opinion,—the opinion I have expressed; but it is a matter which it seems to me the meeting is competent to deal with for itself, and therefore I would move, "That the votes for officers of the Society count as votes for membership of the Council."

Mr. Perigal: I second that resolution.

The President: According to the precedents of this Society it has been usual for such votes not to count, and in consequence of that, certainly on one occasion if not on more, Fellows have lost their position at the Council in consequence of not receiving a sufficient number of votes for some particular office. Of course it would not be in my power to put to the meeting any motion proposed contrary to the Bye-laws, but it is in my power, I believe, to put to the meeting any motion which affects simply the possible interpretation of the Bye-laws. I think the point which has been raised is one upon which there is a difference of

opinion, and upon which those who have legal knowledge if called upon to give an opinion might decide differently. Under those circumstances I shall conceive it my duty on this occasion to put the motion which has been made by Sir E. Beckett. But before I do so it will be open to any Fellows to make any observation they please with reference to it.

The Astronomer-Royal: I should like to ask a question. Suppose the votes are given according to the printed papers, will the votes given for anyone in the capacity of an officer be added to those given to him as a member of the Council?

Sir E. Beckett: Supposing somebody votes for you. Sir George, as a Vice-President and somebody else as Secretary, those of course would not be added for those offices, but if anyone votes for you as a member of Council also, because they wish you to be a member of the governing body, then the votes for Vice-President and Secretary will be added to the votes you would receive as an ordinary member of the Council.

Mr. Ranyard: But a vote for you as Vice-President or Treasurer would not be added to a vote for you as Secretary.

Mr. A. Brewin: I do not see any difference between Sir E. Beckett's proposal and the proposed Bye-law No. 14.

The President: I think it is essentially the same thing, and with this interpretation of the Bye-laws it would seem not necessary to make a new Bye-law for the purpose. Everyone will feel, I think, that the proposal of Sir E. Beckett meets the justice of the case. That no doubt is the way in which the votes ought to count, but one always feels some hesitation in interfering with precedents in a Society of so long standing as ours, and it was for that reason I did not feel myself disposed to move in the matter. But if there is no further discussion I will at once put it to the meeting.

Mr. Gill said the present proposal would simplify the work of the scrutineers. He would suggest that the votes be all collected independently of offices as for the Council, which would include all officers; and then the officers would be elected according to the number of the votes for offices.

Lord Lindsay: That would not quite come to the same thing.

The President: That is a matter for the scrutineers. It is not for us to define exactly the mode in which they shall proceed. They must only act so as to give effect to the motion that is proposed.

Mr. Gill said that Lord Lindsay had evidently misunderstood him. He did not wish the Fellow with the greatest number of votes to be President, and so on; but that those who were nominated to offices should be elected according to the number of votes given in respect of each office.

Mr. Esdaile: I understand that on the last occasion some balloting papers were thrown out altogether because they were signed at the bottom with the voter's name. It was, I believe, decided that the papers contained more than the right number of names, and were consequently invalid; but it seems to me that signed papers should not be rejected for such a technical reason.

Mr. Ranyard said that the scrutineers would necessarily have to decide several points, such as if a name was wrongly spelt they would have to decide to whom it referred, and they must also decide whether a name written at the bottom was the signature of the voter or the name of a Fellow he intended to vote for. These would be matters for their discretion, but he thought they ought not to reject a balloting paper because it was signed.

Dr. Pinches: It is generally understood, I think, that ballot papers should not be signed.

The President: There is no definite statement in the bye-laws authorising the rejection of signed papers, but it is said that the election shall be by ballot, by which it is generally understood that the papers are not to be signed, and therefore any Fellows who may have signed the papers had better cut off their signatures before sending the paper in, so that no signed papers shall be placed in the box.

The motion was then put and carried.

Mr. Neison said: I should like to call the attention of the meeting to the question of publishing the number of votes which the candidates obtain. Hitherto the number has not been published officially, and it was supposed to be a secret, but as a matter of fact it always leaked out, whether correctly or not did not much matter. He thought it a mistake that there should be any secrecy in the matter, and that the Fellows would like to know whether a candidate only got two or three more votes than were necessary to qualify him, or whether he had a large majority, or whether he lost the election by a few votes. He thought it would be an improvement if the numbers were made known, and if a motion was necessary he would move "That the scrutineers be requested to report to the meeting the number of votes obtained by each candidate."

Sir E. Beckett: I second that motion. I think it is the usual course in most bodies. There may be some objections, but I am not aware of any.

Dr. Pinches did not think it would be worth while to introduce an innovation of this kind, and he thought it might create an unpleasant feeling.

Lord Lindsay thought it might be well if the Society knew just how public opinion ran.

Mr. Ranyard said at the last meeting it was understood by the scrutineers that the numbers were not to be published, and he had taken especial care not to tell anybody, but the numbers were published, and that within a few months; he thought, therefore, that if it was so difficult to keep the numbers secret, it would be better that they should be made known in a proper way, and that the scrutineers should have leave to report the numbers to the meeting.

Mr. Mattieu Williams suggested that if the numbers were published they should be limited to those who were elected, because an innocent Fellow might be nominated by another equally innocent Fellow, (laughter) and a small number of votes might make him look ridiculous.

Admiral Ommanney: I am a member of a great many societies, and it is quite unusual to announce the numbers.

Dr. Wentworth Erck: But if you follow the precedent of Parliamentary elections you would give the numbers.

Captain Noble said: On the last occasion Mr. Ranyard absolutely refused to tell him the numbers, and said the matter was a secret, but he found that there were several gentlemen on the Council who knew the numbers. One volunteered to tell him, but never did (laughter), and he found afterwards that they were used for electioneering purposes.

Dr. Pinches: What is done with the voting papers?

The President: They are supposed to be burnt.

Captain Noble: Last year they were taken out of the building altogether.

The President: I may explain the matter. It arose from the circumstance that one of the scrutineers, not being aware of what was the usual custom, which has always been that the papers shall be at once destroyed, took them home with him as the most ready way of at once destroying them. (Laughter.)

Mr. Proctor said: That in the time of Mr. Williams there was a little case into which the papers were put, and for several days after the election it was always possible to examine them. The first occasion on which anything was done in the way of destroying them was after the election of 1873, when, he was informed, they were taken away by one of the scrutineers. He would not mention names, but many would understand whom he meant when he said that that scrutineer was not one to whom he would have applied if he had wanted any information.

Mr. Ranyard suggested that it might be well if instead of destroying the voting papers at once, that they should be placed under lock and key by the scrutineers for a time, in case of the election being disputed. (Cries of "No, no.")

The President: The matter, of course, is in the hands of the Society. There are two points really before the meeting. One is, what is to be done with the papers? It would be better, I think, that they should be burned. (Hear, hear.) Without any formal resolution, I think the scrutineers will carry out the feeling of the meeting, that they should be at once burnt. As to the other matter, upon which Mr. Neison has moved a resolution, my own feeling would be rather against the numbers being published. I think it might give rise to what might be called invidious comparisons. For my own part I should prefer that it be considered a point of honour that the scrutineers do not mention the numbers, and that they should only give us the names which have the highest numbers.

The resolution was then put to the vote and declared lost; thirty-three voting for it, and more than thirty-five against it.

The Astronomer-Royal desired to offer the thanks of the meeting to the President for his very admirable address, and to request his permission that it might be published with the other papers.

Mr. De La Rue seconded the proposal, which was agreed to unanimously.

The Society then proceeded with the Ballot for the Council and officers, and the following gentlemen were declared to be elected:—

President.

Lord Lindsay, M.P.

Vice-Presidents

Arthur Cayley, Esq., M.A., LL.D., F.R.S., Sadlerian
Professor of Pure Mathematics, Cambridge.

Edwin Dunkin, Esq., F.R.S.

William Huggins, Esq., D.C.L., LL.D., F.R.S.

William Lassell, Esq., LL.D., F.R.S.

Treasurer.

Francis Barrow, Esq., M.A.

Secretaries.

J. W. L. Glaisher, Esq., M.A., F.R.S.

A. Cowper Ranyard, Esq., M.A.

Foreign Secretary.

J. R. Hind, Esq., F.R.S., Superintendent of the *Nautical Almanac*.

Council.

J. C. Adams, Esq., M.A., LL.D., F.R.S., Lowndean
Professor of Astronomy, Cambridge.

Sir G. B. Airy, K.C.B., M.A., LL.D., D.C.L., F.R.S.,
Astronomer-Royal.

Sir Edmund Beckett, Bart., LL.D., Q.C.

W. H. M. Christie, Esq., M.A.

David Gill, Esq.

E. B. Knobel, Esq.

George Knott, Esq.

Albert Marth, Esq.

Edmund Neison, Esq.

Captain William Noble.

Richard A. Proctor, Esq., B.A.

Captain G. L. Tupman, R.M.A.

Lord Lindsay took the Chair as the new President, and a vote of thanks to Mr. Huggins, as retiring President, was proposed and carried unanimously. The usual votes of thanks to the retiring officers, members of Council, and Auditors were also proposed and carried. The meeting adjourned at seven o'clock.

THE MADRAS OBSERVATORY.

From several late numbers of the *Madras Weekly Mail*, containing articles concerning and letters from Mr. Pogson, we extract the following. Mr. Pogson complains of being over-worked, so that time and strength have not sufficed for the deduction of results of the observations of seven oppositions of Mars since 1862, and from his numerous observations of planets, comets, and variable stars. He has nearly completed a catalogue of about 3000 stars, observed with the Meridian circle since 1862. He has also in hand an atlas of variable stars, comprising about 130 maps, in which he has been engaged successively at Oxford, Hartwell, and Madras. His daughter has for the last two or three years rendered him able assistance in regard to many of these maps. "This work will most likely have to await publication by permission of the Government of India, after my intended retirement, certainly not more than two years hence at the latest." He has numerous other unpublished works. He says he has served "seventeen years, unbroken by a single day's leave," and implies that, if the Government do not give him "reasonable relief," there will be imminent risk of the total loss of his scientific pursuits, and in some cases their lamentable deterioration in value for want of prompt publication. He has to turn a deaf ear to repeated requests from distinguished European astronomers about casual observations of interesting southern objects he would delight to comply with. The Government has, it seems, requested Mr. Pogson to state the extent and nature of the relief that he requires. The Editor of the *Madras Weekly Mail*, in an article headed "Astronomy in difficulties," thinks Mr. Pogson has attempted too much, and that he is wanting in method and business habits.

In a report from Mr. Pogson to the Madras Government, after observing that Kepler's law, whilst furnishing us with the accurate relative distances of the members of the solar system, gives us by itself a map without a scale, by which to appreciate the actual dimensions of the system, refers to the recent transit of Venus, and goes on to remark that "the planet Mars offers a far better opportunity when the earth comes between him and the sun at successive intervals of about twenty-five and a half months. He is then seen rising at sunset and setting at sunrise, is visible throughout the night, and conspicuously brilliant. At such times he is said to be in opposition, and is by no means at an unmeasurable

distance. If observations are taken from opposite sides of the earth he will be found considerably displaced by our nearly eight thousand mile base line, and his distance may be determined. To measure this displacement with the utmost attainable accuracy requires, however, a long succession of observations extending over several weeks, and can be done in two ways: either by different observers, one as far north and the other as far south as possible; or by one observer near the equator; for it is manifest that if the position of Mars is accurately determined near the time of his rising and again about the time of his setting, diurnal rotation will meanwhile have carried the observer to the opposite side of the earth's centre and have given the widest base line to measure from that the world can afford. This then is the method your astronomer has pursued, night and morning, from August 7th to October 9th, upon every possible opportunity; with what result the future must unfold. But there is every reason to hope with success. Alas that the appliances available in the Madras Observatory are of so inferior an order for the solution of so grand a problem. An expedition for the same purpose was also fitted out by the Royal Astronomical Society under Mr. D. Gill, to the island of Ascension. An excellent heliometer, kindly lent by Lord Lindsay for Mr. Gill's use, and his more favourable southern latitude will probably have secured that astronomer better results than my utmost endeavours here could obtain, but I have done my best with such means as I had, and have little doubt but that either Mr. Gill's observations, costing only a comparative trifle for his voyage to Ascension and back, or my own at Madras, costing not a single rupee extra, will furnish more reliable determinations of the solar parallax than all the prodigious outlay lavished over the popular Transit of Venus in 1874. So long since as April 1857 the Astronomer Royal, when advocating the Mars plan in a lecture at the Royal Astronomical Society, expressed his opinion "that this method is the best of all." After my appointment to my present post, before I left England in January, 1861, and repeatedly since, Sir George Airy has urged my most earnest attention to those observations, and his wishes have ever been most respectfully considered and acted upon in this as in all other matters as far as possible."

The orbit of Mars being very eccentric, it follows that at some oppositions the earth will approach him much closer than at others. These most favourable oppositions recur at intervals of about seventeen years, and the one just past was the best of all for the object in view. I nevertheless believe that even the least favourable oppositions of Mars are better than the transits of Venus, and under that impression have secured similar observations in the years 1862, 1864, 1869, 1871, 1873, and 1875, besides those of the present year."

REVIEWS.

Our Sidereal System, and the Direction and Distance to its Centre. Proceedings of the Academy of Natural Sciences of Philadelphia. 1876. By Jacob Ennis.

The conclusions of the writer are—1. That the galaxy is of a ring form. 2. The centre of gravity is the centre of the ring. 3. All the stars of our Sidereal System revolve with high velocities around its centre of gravity. 4. Our own position is on the north side of the galactic plane about 21° from it. 5. The distance from our own position to the galactic centre, or centre of gravity of our sidereal system, is such that it requires light 87 years to pass between them; and, therefore, the centre of our sidereal system is distant from us about as far as stars of the fifth magnitude. Mr. Ennis also says, 1400 years are required for the passage

of light from the more distant to the nearer stars of the galaxy; and that a star moving at the rate of 1000 miles per minute—about like that of our earth around the sun—requires 150,000,000 years for one revolution around the sidereal centre.

On the Visibility of the Dark Side of Venus. By Prof. A. Schafarik, of Prague. (From the Report of the British Association for the Advancement of Science for 1873).

Prof. Schafarik has collected as many as twenty-two observations of this phenomenon, from that of W. Derham, 1729 (or before?) to that of Dr. A. Winnecke, of Karlsruhe, 1871. The others being by C. Kirch, A. Mayer, Sir W. Herschel, Count F. Hahn, Schröter, C. L. Harding, J. W. Pastorff, Gruithuisen, Guthrie, G. A. Jahn, Mr. Berry, C. L. Prince, W. Engelmann, Prof. C. S. Lyman, Mr. Th. Petty, Prof. Schafarik, Mr. R. Langdon, Capt. W. Noble, Mr. Browning, Dr. A. Winnecke. By these twenty-two observers the unilluminated side of Venus was seen in twilight by thirteen (once by four, many times by nine). In daylight by eleven (once by six, many times by five). Four observers saw a faint line of light encircling the dark disc, nineteen of them saw the disc itself. Different explanations of this phenomenon are then enumerated. 1. *Phosphorescence* (Sir W. Herschel, Harding, and partly Schröter). 2. *Auroral phenomena* (partly Schröter's idea, and strongly advocated by Prof. Zöllner). 3. *Proper light* (Pastorff). 4. *Light of the earth* (shown by calculation to be an insufficient explanation). 5. *Negative visibility*, or projection on the coronal light of the sun. (Mr. Lynn, Noble?). But the majority of observers make the unilluminated side brighter than the sky. 6. *Accidental combustion and other illuminating processes*, (Gruithuisen). Prof. Schafarik himself alludes to the intensity of the phosphorescence not unfrequently seen in our tropical seas; and if the present condition of Venus be analogous to that of the earth in the Jurassic period, when large isolated islands were bathed by immense seas, blood-warm, and teeming with an abundance of animal life, he suggests that a phosphorescence greater than in our own seas might possibly be seen even at planetary distances.

METEORS.

On the night of the eclipse (Aug. 23) a ball of light of the apparent size of the moon, was seen at ten minutes to 11, dropping apparently from cloud to cloud, and the light flashing like lightning across the road. On the 25th another was seen at past 8 o'clock, but smaller, which burst and scattered its parts about in space.—(From Countess Baldelli, Florence. See *Astronomical Register*, vol. III.)

ARTIFICIAL HORIZON.

From a paper by Mr. Reuben Burrow, in the *Asiatic Researches*, Calcutta. Vol. I. (last century).

"The utility of a perfect horizon, and the liableness of quicksilver to be disturbed by the least wind, have induced numbers of people to invent artificial horizons of different kinds, and many of them very complicated. * * * I tried a number of other methods with little success. * * * At last, * * * I applied some mosquito curtain (a kind of silk gauze, as close as book-muslin, and perfectly transparent. It is to be stretched over a hoop, which stands without touching the vessel containing the mercury,) as a covering to the quicksilver, and found it effectually excluded the wind and admitted the sun; and what is of

equal consequence in this country, it totally kept away those minute insects that disturb the surface of the quicksilver in observing: in short, it formed so complete an horizon, that I could not before have hoped for anything so perfect; and it is equally applicable to the sun and stars."

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

A PORTABLE EQUATORIAL MOUNTING FOR SMALL TELESCOPES,

ADAPTED FOR THE USE OF TRAVELLING ASTRONOMERS OR SEA-SIDE VISITORS.

This mounting packs into a case $9 \times 4\frac{1}{2} \times 1$ inch, and yet is quite strong enough to carry a telescope $3\frac{1}{2}$ inches aperture, and 50 inches focus.

It consists of a small cast-iron parallel vice, or clamp, which can be attached to any rail, vertical or horizontal, *e.g.*, to any window sash, French or English. At one corner of this clamp is a cube of iron 1 inch each way, and into any face of this cube screws a small vertical axis round which revolves a small frame carrying the polar axis after the German fashion, and thus admitting of adjustment to any azimuth. If desired a joint can be introduced into the frame between the vertical and polar axis, so as to admit of adjustment for latitude, but in the present instance this was not required.

For convenience of packing, that part of the declination axis which carries the counterpoise unscrews just outside the bearing.

W. ERCK.

DARK SIDE OF VENUS.

Though a frequent observer of the planet Venus through a long series of years, I have never till yesterday evening seen the unilluminated side, which presented itself rather unexpectedly, as I had not been thinking particularly about it, and was not making it an object of special examination. The air was frosty and hazy, and definition tremulous, and the planet low; my beautiful $9\frac{1}{2}$ -inch "With" mirror brought nevertheless the horns to sharp points. I had noticed nothing remarkable with a low power Kellner eye-piece, but on changing it for 212 I perceived the phenomenon pretty distinctly at intervals; it was much overpowered by the splendid light of the planet, but came out occasionally rather paler and browner than the background of the twilight sky. It was not visible with Browning's E eyepiece of about 357, but was fairly distinguished with a home-made positive eye-piece giving a power of perhaps 90 or 100, and was equally perceptible when the planet was hidden by a bar in the field. It seemed, as has been noticed before by Schröter, and I believe others, to be part of a smaller circumference than that of the enlightened limb.

Hardwick Vicarage: Feb. 1, 1878.

T. W. WEBB.

ω^1 CANCRI.

This star was announced by Mr. Buckingham at a meeting of the Royal Astronomical Society in 1872 to be a double star. In the *Astronomical Register* for November, 1874, I stated that I had not been able to see the slightest trace of any companion with my 6-inch refractor, and requested any correspondent who had seen it double to give particulars of the observation. Mr. George Knott in the *Register* for April, 1875, copied an entry from his observing book of April, 1872, giving the estimated position angle 340° , and distance $2''$; companion $11\frac{1}{2}$ magnitude, and colours golden and grey or greyish blue. I have recently learned from Mr. Knott that he has failed to see in a late examination any indication of the small star, and is unable to account for his former observation. I have examined this star carefully under favourable conditions with the $18\frac{1}{2}$ -in. refractor, and I am certain no such attendant exists. Certainly some other star was observed by Mr. Buckingham and probably the same by Mr. Knott. The chances appear to be in favour of this being Cancri 5 (Σ 1171), a pair about $1^\circ 50'$ south. Struve gives the angle $338^\circ 6'$, and distances $2''\cdot 80$, magnitudes, $6\cdot 2$ and $10\cdot 7$, the colour of the larger being, "flava." It will be seen that this corresponds to the description of the other in every particular, and there cannot be much doubt it is the star usually observed for ω Cancri.

Chicago: Jan. 30.

S. W. BURNHAM.

JUPITER'S ATMOSPHERE.

Sir,—I cannot spare time for the discussion of this subject. I could not even if Mr. Neison urged arguments instead of merely repeating statements.

I may simply explain for the benefit of some among your readers, that the empiricism of which I before spoke, extends to (or rather begins with) the laws connecting pressure, density, temperature, etc., on which the formulæ used by Mr. Neison are based. These laws have been established by experiment carried on under particular conditions; but they are not all true under all conditions, and some of them are found not even to be true within the comparatively narrow range of experimental research. As an illustration of the nature of such laws, we may take Newton's law of temperatures. This law is true within the limits of Newton's experiments. Far outside these limits it is not true, and we must replace it by the law of Dulong and Petit, and this law, in turn, is not universally true: for instance, it cannot be relied upon to give us the true temperature of the sun's surface.

Mr. Neison is mistaken in supposing that I overlooked the general form of the equations relating to atmospheric refraction. The question considered was not whether Ivory's equations, or Laplace's, or Lubbock's, were to be preferred, but which of those mathematicians Mr. Neison had chiefly consulted. I am not at all surprised to find that he has consulted all of them, Ivory, Kramp, Laplace, Bessel, Plana, Lubbock, and Cœteri, to obtain formulæ which a mathematician taking up this special subject would, I should think, prefer to obtain for himself.

I was aware also that Bessel (who was stronger in mathematics than in physics) had investigated the case of lunar atmospheric refraction in the unsatisfactory manner referred to by Mr. Neison. If the weight of Bessel's name is to decide a question, I wonder why Mr. Neison should have re-examined that particular subject. I forget what result he obtained, but if it was the same as Bessel's, his inquiry was scarcely worth publishing. If he obtained a different result he must, I suppose, believe

that Bessel might err like the rest of us (unless, indeed,—which the tone of his letter suggests—an exception is to be made in Mr. Neison's favour).

But although I chance to have read this particular investigation of Bessel's, I make no doubt Mr. Neison might easily name many papers which he has studied and I have not. (His remarks about myself remind me of Leech's very young lady who at a social gathering announces loudly to her mother, "Mama, I asked Miss Brown and Mr. Robinson if they knew in what year or century Aristarchus of Samos flourished, and they could not say, but I can,—and—and Anaxagoras and Anaximander flourished in the etc., etc., etc.") I have never doubted the extent of Mr. Neison's reading; whereas, for my own part, I have generally found it takes less time to deal with mathematical matters directly than to hunt out the formulæ obtained by others. As I suppose Mr. Neison thinks it in good taste to comment on the presumed extent of other persons' reading, he can hardly object to my remarking that the student of science should not read so much as to be left scant time for thinking.

I have noticed Mr. David Trowbridge's paper. I glanced over the first few lines and noticed that it was based on unsound data, so that I am not surprised to learn that the deductions were unsound also. A similar remark applies to Mr. Neison's article on Jupiter.

RICHARD A. PROCTOR.

Sheffield, Feb. 5, 1878.

SPECULAR REFLECTION FROM VENUS.

Sir,—Your report on p. 40 of what I said at the meeting of the Royal Astronomical Society on January 11th very curiously misrepresents the meaning of the few remarks I really did make.

What, in effect, I said was, that Mr. Christie's recent observation of a minute point of light within the limit of Venus, could in no sense be held to corroborate the existence of the sausage-shaped patch of light seen last year by Mr. Brett; and, in fact, that the production of such a sausage-shaped image by the reflection of the sun's disc from a polished metallic sphere was absolutely inexplicable on any known optical principle whatever.

Forest Lodge,
Maresfield, Uckfield,
Feb. 12, 1878.

I have the honour to be, Sir,
Most obediently yours,
WILLIAM NOBLE.

STAR σ^2 PISCUM.

Sir,—On looking up this object lately, I found its companion is easily glimpsed with a $2\frac{1}{4}$ inch achromatic, so that its magnitude cannot be reckoned lower than 9. Smyth in his Cycle gives 10½. Judging also by eye estimate, the angle of position is somewhat different to what he states. It is a pity this object is not included among the double star measurements given in the recently published number of the R.A.S. Memoirs.

Upton Helions Rectory :
Crediton.

S. J. JOHNSON.

COMETS.

Sir,—Just twelve years ago, viz., in March, 1866, you published a short letter of mine commencing as follows :—

"The spherical nucleus of a comet when it is transparent, although it is of very small actual density, is denser than the surrounding medium,

and must therefore form a spherical lens; and the halo round the nucleus and the tail are merely optical phenomena produced by the sun shining through such nucleus or spherical lens. The tail must be (as it actually is) always in the direction of a ray from the sun, and if a *non-transparent* nucleus become suddenly transparent the tail would be projected with the velocity of light (of this instances are on record)."

There are but very few words in the above I should desire in any way to change or modify.

Of course I did not then know that the greater part, if not the whole, of a comet's path is filled with cosmical matter (or star-dust if that expression be preferred), so that I was at that time quite unable to explain the curvature of the tail.

With our present knowledge the curvature of the tail is the exact phenomenon we should expect: the light of the sun shining through the nucleus illuminates the particles where they exist, and they extend along the path of the comet, that is, in a curved form.

We must remember that the gas, of which the nucleus (according to the above theory) is formed, is certainly not more than one-millionth part as dense as common air; that is, a comet a million of miles in diameter may offer no more resistance to the sun's rays than (in other words, may be as transparent as) a mile of atmospheric air.

The objection now likely to be insisted on is the difficulty of explaining the various phenomena connected with what I should call the halo: I shall be asked how I can explain that there are sometimes as many as three domical or mushroom-like appearances in front of the nucleus.

I imagine these all to be phenomena of reflection, assisted no doubt by the changing position of the cosmical matter: it is probable that the gas, of which the nucleus is composed, is always changing its density and its form, and thereby perpetually producing these very rapidly shifting appearances.

The above theory explains why we can obtain absolutely no weight from a comet. I remember the late Professor De Morgan telling me that he, when presenting a gold medal for an essay on comets, had stated in the presence of the principal astronomers that no one there present would venture to assert that all the comets ever seen had weighed as much as the piece of gold in the medal.

The comet (that is the nucleus), merely consists of a certain number of the particles of matter with which the whole path is filled, that from some cause have assumed a gaseous form, so that there is no reason why the comet should weigh any more than any other portion of the path.

I believe this theory is steadily gaining ground; indeed, I fail to see that any really valid objection has been raised against it.

I remember that one of your correspondents asserted in 1866 that Tycho Brahe had made some suggestion about a comet being a lens; I should be very glad if any of your readers could give us the exact words of Tycho.

I am, sir, yours, etc.,

6, Lancaster Gate, W.:

EDWIN LAWRENCE.

Feb. 11, 1878.

CHANGE OF COLOUR IN *a* URSI MAJORIS.

Sir, - Herewith I beg to forward to you, thinking it possibly may be held suitable for the pages of the *Astronomical Register*, a translation of two short articles upon an asserted periodic remarkable change of colour exhibited by the star *a* of the Great Bear. Dr. H. J. Klein, whose attention was originally drawn thereto fifteen or sixteen years ago, is at present the editor of the German Quarterly Review for Natural Sciences.

The changes of colour alleged to occur are so striking, and the period within which they are stated to be comprised is so short, that a moderate amount of attention given to the star in question during a few months would probably suffice for establishing or refuting Dr. Klein's statement.

Should what he maintains to be the case be borne out by the observations that are made, the inference drawn by Mr. Tromholt in the second article forwarded to you seems to follow as a matter of course.

I am, sir, yours truly,

W. G. LETTSOM.

Dec. 27, 1877.

ON THE CHANGES THAT OCCUR IN THE COLOUR OF THE STAR α OF
THE GREAT BEAR.

As long ago as in 1862, Dr. Hermann J. Klein, as the result of a great number of observations made with every care, was led to recognize that the star α of the Great Bear undergoes with regularity, in a period of four weeks and a half, a change of colour from intense fire-red to yellow.

Mr. H. Weber, at Peckeloh, who is well known from his careful observations of the sun during a course of several years, has recently occupied himself with noting the changes of colour observable in the star here spoken of.

With this view he employs a telescope furnished with a power of 90, inasmuch as the brightness of the star is increased thereby, and the estimation of its colour facilitated in consequence. The following is the series of Mr. Weber's observations up to the present time:—

1876. Date.	Colour of Star.
Aug. 22	Whitish yellow.
24	Pale yellow.
25	Yellow.
28	Golden yellow.
29	Reddish yellow.
Sept. 5	Marked fiery red.
16	Reddish yellow, faint.
19	Golden yellow.
21	" "
29	" rather duller.
Oct. 3	Yellow.
5	Strongly yellow.
6	"
7	Strong golden yellow.
9	" reddish yellow.
10	Deep fire-red.
11	Deep yellow.
12	"
13	Light yellow.
18	Pure yellow.
19	"
21	Plain yellow.
24	Yellow, pale bluish
28	White.
31	Whitish yellow.
Nov. 6	Yellow.
10	Golden yellow, dull reddish.
14	Fire-red, somewhat dull.

1876. Date.	Colour of Star.
Nov. 21	Whitish yellow.
26	Pure yellow.
Dec. 3	Whitish yellow.
5	Dull yellow.
14	Reddish yellow.
21	Fire-red, somewhat dull.
26	Golden yellow.
30	Yellowish white, white rather.
1877.	
Jan. 2	Yellowish white.
5	Yellow.
7	"
9	Golden yellow, bright.
15	Dull red.
16	Fire red.
18	Golden yellow, bright.
23	Yellow, whitish.
30	Pure yellow.
31	Bright yellow.
Feb. 1	Whitish yellow.
4	Whiter, only faintly yellowish.
8	Quite white, pale yellow.
13	Pure yellow.
15	Golden yellow.
16	Golden yellow, faintly reddish.

As will be noticed, these observations by Mr. Weber support entirely those previously carried out by Dr. Klein, indeed certain peculiarities noticed by the latter as much as fifteen years ago in the course of the changes of colour are reproduced in Mr. Weber's observations. What we have to deal with then is a change of striking regularity, and it is singular that in the first positively recognised instance of a periodical change of colour, the entire range should be so distinctly traceable. From Mr. Weber's observations there obtained with regard to *red*, a period of 33.2 days, with respect to *white* one of 33.8 days, giving, therefore, an average of 33 days and a half. No stricter accordance in establishing the tone of the colour could well be looked for.

This change of colour, to use Mr. Weber's words, "repeats itself with such regularity that I can already predict with something like certainty when the star will present itself as a *red* one and when as a *white* one."

The observation of this change of colour, which may be carried out with facility even with the help of an opera-glass, is a matter to which the attention of the friends of astronomy may with propriety be directed. (Translated from *Sirius*, Vol. V., New Series, p. 58, No. 3, 1877.)

ON THE OBSERVATION OF α OF THE GREAT BEAR.

From Dr. H. J. Klein's discovery of the change of colour of α of the Great Bear, which is supported by the observations of Weber, I have been led to an inference, which, however, unfortunately, from the want of the suitable instrumental means, I am not in a position to submit to the test of a trial myself, but which I make bold to submit to you in the following form:—

If a star undergoes changes of colour, its spectrum too must exhibit change.
Whence it follows:—

Firstly.—That the spectroscopic observation of stars that are variable in colour—the examination of *Dubhe* in that manner to begin with—will bring to light interesting and hitherto not noticed facts in spectroscopy.

Secondly.—That the discovery of such stars and the determination of their periods will be essentially facilitated by the employment of the spectroscope.

SOPHUS TROMHOLT.

Bergen, Norway:

May 30, 1877.

(Translated from *Astronomische Nachrichten*, No. 2,139. Kiel, 2 July, 1877.)

"HONOUR TO WHOM HONOUR"—EXPLANATORY NOTE.

When speaking of the spectroscope, at the last Evening Meeting of the Astronomical Society, I used the words—

"It may interest some present to hear of certain improvements introduced into a spectroscope, made by Mr. Grubb, for Professor Young"; thus purposely leaving it open as to whose improvements they were: for at the time of speaking I knew they were due to more than one person, but to whom, and in what degree to each, I knew not.

My words having been misunderstood, Mr. Grubb desires me to state what is an undoubted fact; viz., that he had long before disclaimed to me any personal merit in the principle of these improvements, and that the sole merit of the automatic arrangement is due to Professor Young himself.

Feb. 4, 1878.

WENTWORTH ERCK.

ASTRONOMICAL OCCURRENCES FOR MARCH, 1878.

DATE.		Principal Occurrences.	Jupiter's Satellites.	Meridian Passage.
		h. m.		h. m. s.
Fri	1	Sidereal Time at Mean Noon, 22h. 36m. 37 ^s . 57 ^s . Reappearance of Saturn's Ring		Regulus 11 23' 4"
Sat	2	0 8 Conjunction of Moon and Venus 8° 47' N. Conjunction of Moon and Mercury 2° 48' S.		11 19' 5"
Sun	3	Sun's Meridian Passage 12m. 7 ^s . 58 ^s . after Mean Noon 15 17 ● New Moon	2nd Tr. I. 17 53	11 15' 5"
Mon	4	13 Conjunction of Moon and Saturn, 5° 8' S.		11 11' 6"
Tues	5			11 7' 7"
Wed	6	Saturn's Ring: Major axis=35° 45' Minor axis=0° 17' 5 44 Occultation of 101 Piscium (6) 6 54 Reappearance of ditto	1st Ec. D. 17 54 23	11 3' 8"
Thur	7	10 2 Occultation of 20 Arietis (6) 10 48 Reappearance of ditto	1st Sh. E. 17 32 1st Tr. E. 18 31	10 59' 8"
Fri	8			10 55' 9"
Sat	9	Conjunction of Moon and Mars 4° 16' S.	2nd Sh. E. 17 41	10 51' 9"
Sun	10	13 16 Near approach of χ^1 Tauri (5½)		10 48' 0"
Mon	11	16 0 ☾ Moon's First Quarter		10 44' 1"
Tues	12			10 40' 1"
Wed	13	10 29 Occultation of 52 Geminorum (6) 11 34 Reappearance of ditto 6 Conjunction of Saturn and Sun		10 36' 2"
Thur	14	7 26 Near approach of μ^2 Cancri (5½) 13 14 Occultation of B.A.C. 2788 (6) 14 7 Reappearance of ditto	1st Sh. I. 17 6 1st Tr. I. 18 10	10 32' 3"
Fri	15	Illuminated portion of disc of Venus=0.140 Illuminated portion of disc of Mars=0.915	3rd Oc. R. 17 15 1st Oc. R. 17 38	10 28' 3"

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Sat	16	9 57	Occultation of α Leonis (5)			Regulus
		10 36	Sideral Time at Mean Noon 23h. 35m. 45 ^s .878			10 24'4
Sun	17	8 10	Occultation of δ Leonis (5)			
		9 4	Reappearance of ditto			10 20'5
		5	Conjunction of Saturn and Mercury $0^{\circ} 17' N.$			
Mon	18	9 6	O Full Moon Sun's Meridian Passage 8m. 11 ^s .908. after Mean Noon			10 16'5
Tues	19	8 33	Occultation of B.A.C. 4312 (64)	2nd Ec. D.	16 21 11	10 12'6
		9 22	Reappearance of ditto			
Wed	20	16	Superior conjunction of Saturn and Sun			10 8'7
Thur	21					10 4'8
Fri	22	13 18	Occultation of γ Scorpii (6)	1st Ec. D.	16 10 20	10 0'8
		14 10	Reappearance of ditto	3rd Ec. R.	16 45 35	
Sat	23			1st Tr. E.	16 57	9 56'9
Sun	24	15 1	Occultation of B.A.C. 6072 (64)			9 52'9
		15 23	Reappearance of ditto			
Mon	25	4 49	☾ Moon's Last Quarter			9 59'1
Tues	26		Saturn's Ring : Major axis = $35^{\circ} 49'$ Minor axis = $0^{\circ} 88'$			9 45'1
Wed	27	9	Conjunction of Moon and Jupiter $1^{\circ} 31' N.$			9 41'1
Thur	28			2nd Sh. E.	16 0	9 37'2
Fri	29	14	Conjunction of Moon and Venus $3^{\circ} 30' N.$ Venus at greatest brilliancy	3rd Ec. D.	17 30 18	9 33'3
Sat	30			1st Tr. I.	16 35	9 29'3
Sun	31			1st Oc. R.	16 3	9 25'4
APRI	.					9 21'5
Mon	1					

THE PLANETS FOR MARCH.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	21 57 28	S. 14 43	5"·0	23 17·0
	9th	22 49 58	S. 9 44	5"·0	23 37·9
	17th	23 44 46	S. 3 25	5"·0	* * *
	25th	0 34 52	N. 2 59	5"·0	0 23·6
Venus ...	1st	21 46 24	S. 4 27½	57"·4	23 6·0
	9th	21 38 46	S. 6 14	53"·1	22 26·9
	17th	21 41 10	S. 7 31	46"·9	21 57·9
	25th	21 56 22	S. 8 8	39"·1	21 37·6
Mars ...	1st	3 3 30	N. 18 22½	6"·8	4 26·1
	9th	3 23 60	N. 19 45½	6"·5	4 15·1
	17th	3 44 52	N. 20 59½	6"·2	4 4·5
Jupiter ...	26th	20 17 34	S. 19 59	33"·6	19 59·1
Uranus ...	2nd	9 57 10	N. 13 19½	4"·2	11 14·8
	18th	9 54 46	N. 13 32	4"·2	10 9·4

Mercury is badly situated for observation during greater part of the month. He sets about half an hour after the sun towards the end.

Venus may be seen for an hour and a quarter before sunrise at the beginning of the month, the interval increasing.

Mars sets near midnight throughout the month.

Jupiter is visible an hour and a half before sunset on the first day, the interval increasing.

SATELLITES OF URANUS.

Major and minor semi-axes a and b of the apparent ellipses described by the satellites, and position-angles p_0 of the major axes:

1878.	Ariel.		Umbriel.		Titania.		Oberon.		p_0
	a	b	a	b	a	b	a	b	
Mar. 6	15".20	4".93	21".18	6".87	34".74	11".26	46".45	15".06	11°.41
16	15".15	5".00	21".10	6".97	34".61	11".44	46".28	15".30	11°.31
26	15".07	5".06	20".99	7".05	34".43	11".56	46".04	15".46	11°.21

The satellites will be at their greatest elongations N. (in position angle $11^\circ.3$) and S. ($191^\circ.3$), and at their superior ($101^\circ.3$) and inferior ($281^\circ.3$) conjunction with the planet, about the following hours, G.M.T.:

Ariel.			Umbriel.			Titania.			Oberon.		
1878.	h.		1878.	h.		1878.	h.		1878.	h.	
Mar. 1	20	S.	Mar. 1	6	S.	Mar. 1	0	N.	Mar. 2	8	S.
3	2	N.	3	8	N.	3	4	inf.	5	17	sup.
4	8	S.	5	9	S.	5	8	S.	9	2	N.
5	14	N.	7	11	N.	7	12	sup.	12	10	inf.
6	21	S.	9	13	S.	9	17	N.	15	19	S.
8	3	N.	11	15	N.	11	21	inf.	19	4	sup.
9	9	S.	13	16	S.	14	1	S.	22	13	N.
10	15	N.	15	18	N.	16	5	sup.	25	22	inf.
11	22	S.	17	20	S.	18	10	N.	29	6	S.

13	4 N.	19	22 N.	20	14 inf.	April 1	15 sup.
14	10 S.	21	23 S.	22	18 S.	5	0 N.
15	16 N.	24	1 N.	24	22 sup.		
16	23 S.	26	3 S.	27	3 N.		
18	5 N.	28	5 N.	29	7 inf.		
19	11 S.	30	6 S.	31	11 S.		
20	17 N.	April 1	8 N.	April 2	15 sup.		
22	0 S.						
23	6 N.						
24	12 S.						
25	18 N.						
27	1 S.						
28	7 N.						
29	13 S.						
30	19 N.						
April 1	2 S.						

In the *Astronomical Register* for January and February the values of p_0 have, by mistake, been wrongly given—

8°13	on Jan. 25	instead of	11°87
8°24	Feb. 4	"	11°76
8°35	" 14	"	11°65
8°47	" 24	"	11°53

or as much below 10° as they should have been above.

A. M.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN MARCH, 1878.

By W. R. Birt, F.R.A.S., F.M.S.

OBJECTS SURROUNDING PLATO. It may contribute greatly to the interest of the examination of these objects now in progress, if a comparison be made with the observations of 1860 and 1861 recorded in MS. and deposited in the library of the R. A. S. They are arranged in the order of the moon's age, which will enable observers to look for features which were recorded in those years. The *earliest* observation of the series according to this arrangement, Moon's age, 7·24 days, was made in 1860, July 25, 8°0 to 8°30, G. M. T., Terminator among the Alps. The whole of Plato still in darkness.

Moon's age, 7·57. 1860, April 28, 7°30 to 7°45. Terminator a little eastward of the western extremity of Plato, the S.W. rim of Schröter's Newton visible.

N. wall, summit appears as *four* lucid points.

E. " " not visible.

S.E. " " not illuminated.

S. and S.W. appears on the *exterior* as double, with a dark mark between; this mark is a ravine on the surface exterior to and on the S.W. of Plato. A little ruggedness is seen eastward of this double portion.

W. wall, summit appears apparently broken by mountains in the neighbourhood. Floor in perfect darkness.

At 7°45 the Terminator passed through the centres of Plato and Newton. Pico on the rim of Newton appeared to possess a double summit.

N.W. wall bright and rugged.

S.E. " two points of light now visible, the outline of the ring very discernible. Floor still in perfect darkness.

Moon's age, 7.67. 1860, June 26, 9.15. Terminator across the centre of Plato, just eastward of Pico, and eastward of the mountain mass S. of Pico, B of B. and M., and about 0.75 of the length of Schröter's Newton over it, measured from the west.

N. wall, summit *four* lucid points.

E. " " The rock ζ of B. and M. very brilliant.

S.W., " " The ravine on the exterior very distinct. Floor in perfect darkness.

Moon's age, 7.73. 1861, July 15, 8.0. Terminator at 8.20 crossing the western portion of the floor of Plato.

N. wall, summit a series of bright heads.

E. " " the rock ζ very bright.

W. " " a prominent rock δ on the west margin of Plato in a line with ζ.

S.E. summit becoming illuminated.

S.W. " " The ravine on the exterior very distinct. The site of Schröter's Newton very apparent. Pico brilliantly illuminated.

The two rocks S.W. and N.W. of Plato, λ and ν, very finely illuminated, casting well-defined shadows. Two well marked and distinct rocks in front, *i.e.*, N.W. of ν, seen.

Buckhurst Hill: Feb. 14, 1878.

Books received.—Annual Report of the Director of the Harvard College Observatory. Cambridge: John Wilson and Son. 1877.—The Spectroscope. By John Browning. London: John Browning.—Myths and Marvels of Astronomy. By R. A. Proctor. London: Chatto and Windus, Piccadilly. 1878.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1877.

Lawton, W.

Linwood, Rev. W.

To Jan., 1878.

D'Alquen, Mrs.

To Feb., 1878.

Allen, A.

To March, 1878.

Lewis, H. K.

To June, 1878.

Jones, W. E.

Ward, J. W.

To Aug., 1878.

Burton, C. E.

To Dec., 1878.

Banks, W. L.

Gould, Rev. R. J.

Johnson, R. C.

Langley, Prof. S. P.

Loddiges, C.

Dr. D. v. Monckhoven.

Perigal, H.

Wilson, T.

To June, 1879.

Basley, T. S.

Perry, Rev. S. J.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for the Editor are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

TRANSIT INSTRUMENT WANTED.—Any person having an efficient Transit Instrument (Second hand) to dispose of, will oblige by forwarding particulars to Mr. G. F. RADDIFORD, Barnwood Lodge, Gloucester.

The Astronomical Register.

No. 184.

APRIL.

1878.

ROYAL ASTRONOMICAL SOCIETY.

Session 1878—79.

The First Meeting of the new Session, March 8th, 1878.

Lord Lindsay, M.P., &c., *President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

John Ellard Gore, Esq., C.E., M.R.I.A., Assistant Engineer

D.P.W. India, Dromard Rectory, Co. Sligo.

Rev. Frederic John Hall, Haileybury College, Hertford.

William Hobson, Esq., Park View, Whalley Range, Manchester.

Lieut. Robert Hoggan, R.N., H.M. Dockyard, Sheerness.

William Curtis Otter, Esq., 60, Grosvenor Park, Camberwell.

F. H. Stevens, Esq., B.A., Clifton College, Bristol,

were balloted for and duly elected Fellows of the Society.

Mr. Glaisher announced that 84 presents had been received since the last meeting.

Mr. Chambers said: Before we pass to the ordinary business, I want to throw out a suggestion as to whether some arrangement cannot be made for expediting the publication of the *Monthly Notices*. It strikes me as inconvenient that we should not receive the *Monthly Notices* until the day of the following meeting. I know that on a former occasion when complaint was made at Somerset House, the defence put forth by the Editor was that gentlemen did not return the "proofs" in time. Now, supposing that to be the cause of the delay, I think those gentlemen ought to be fined to the extent of having their communications post-

VOL. XVI.

poned, rather than that the general convenience of the whole body of members should be sacrificed to the dawdling propensities of two or three members.

Mr. Ranyard: This number contains the annual report, and it is no doubt rather exceptionally late owing to the difficulty in getting some of the "lives" completed, but during the last few months the numbers of the *Monthly Notices* have been published much earlier than they formerly were.

Capt. Noble: I can heartily endorse all my friend Mr. Chambers has said. I have asked Mr. Cayley on several occasions at the Council why the number has not been sent out earlier, and I think the fault has generally been laid upon the printers rather than upon the gentlemen who will not correct the proofs. The last two or three numbers have been a long time in reaching me. I have usually to read them coming up to town in the train. If there is anything in the *Notices* which would involve discussion at our meetings we ought to have at least 24 hours to consider it. I do not know whether a hint to that effect will have any good result. There are reporters in the room, and perhaps if the fact goes forth that the Fellows would be better satisfied with more expedition, it may wake somebody up—but I will not say whom. (Laughter.)

Mr. Lynn: I think we must allow that the case is rather exceptional this time, considering the great length and value of the report.

Mr. Chambers: Unless my memory fails me, certainly during three out of the last five months my *Notices* have not reached me till the Friday morning of the meeting day, and I have only been able to read them coming up in the train on the morning of the day.

The President: We will now pass to the ordinary business of the Society.

Mr. Neison explained the substance of a paper *On Hansen's terms of long period in the lunar theory*. It would be remembered, he said, that about three years ago the Astronomer-Royal, in a paper read before the Society, announced that a certain term of long period in the motion of the moon, discovered by Hansen, ought to be omitted for the future from the lunar theory, upon the ground that the later investigations by Delaunay and Prof. Newcomb had shown that its value was insensible. A short time back Prof. Newcomb published a paper in the *American Journal of Science*, in which he made an elaborate investigation of the corrections of Hansen's tables of the moon, and showed that though Hansen was able, by certain artifices to make his tables represent the motion of the moon for the period between 1750 and

1850, yet for periods anterior and subsequent to this time Hansen's tables gave places which deviated from the true position of the moon. It was very difficult on any hypothesis to explain these discordances, and of late years Hansen's tables were far from truly representing the place of the moon.

While making some researches upon certain points of the lunar theory, Mr. Neison said that he was led to examine Delaunay's investigation, and found that, though it was perfectly correct so far as it went, yet he had entirely omitted certain classes of terms from his equations, which might yield a sensible result, although he had fully considered what would be the effect of the action of the planet Venus on the motion of the moon, Delaunay had failed to consider how the action of the sun would be modified in accelerating or retarding the moon in its altered orbit. He has shown that the action of Venus will cause the moon to move in a somewhat larger orbit for the space of about 50 years, and then in a smaller orbit for a similar period. It would be evident that, when the moon was moving in the larger orbit, the earth would have less effect upon it, and consequently the perturbing influence of the sun upon the motion of the moon would be greater. Delaunay, in considering the perturbing influences of the sun upon the motion of the moon had omitted to take into account the variations of the moon's orbit caused by the action of Venus, and had assumed that the effects would destroy each other. Mr. Neison's paper was intended to show that it was possible that the action of the sun upon the alternately larger and smaller orbit of the moon, might be sufficient to account for the term of long period for which Delaunay had been unable to find any explanation, when he considered the moon as moving in a fixed orbit, and neglected the effect of Venus.

Mr. Drach : May I ask Mr. Neison how much the orbit is enlarged ?

Mr. Neison : I am afraid I cannot say off-hand.

Mr. Drach : I mean how much would the major axis of the moon's orbit be enlarged by the action of Venus ?

Mr. Neison : The increase would be very small, perhaps not amounting to $\frac{1}{30}$ th of a second of time for the year, but it goes on for 50 years, and at the end of that time it may be two seconds of time.

The President : I will ask you to return your thanks to Mr. Neison for his paper.

Mr. Proctor was called upon to read a paper *On the determination of the axial position of Mars with respect to the earth at any epoch*. He said that he had intended to make use of the observations of Mr. Green to prepare a new chart of Mars to submit to

the meeting, but that he had found that the work would be greater than he had expected, and he had therefore been obliged to defer the chart till another occasion. It seemed to him that Mr. Dawes, though one of the most acute of observers, was not very skilful in delineating upon paper what he saw. There was a great amount of work in going over and correcting a chart of Mars, and the paper that he was about to explain was intended to show that one might form projections of Mars by making use of the right ascension and declination of the planet given in the *Nautical Almanac*, in cases where tabulated results, like those on which Mr. Marth had expended so much labour, were not available.

Mr. Proctor gave the following construction for drawing the polar axis and equator of Mars from the known right ascension α and declination δ at any epoch, I . (about $39\frac{3}{4}^{\circ}$) being the inclination of the equator of Mars to the terrestrial equator, and N . (about $47\frac{1}{2}^{\circ}$) the right ascension of the rising node of the former plane on the latter. Let $P O P'$, $E' O E$ be diameters intersecting at right angles of the circle $P E P'$, take arc $P p = I$, and draw the chord $p c n$ perp. to $P P'$ cutting it in c . From p take arc $p l$ of $\frac{1}{2}$ circle $p l n$ about c as centre $= \alpha - N$, and draw $l q$ perpendicular to $p n$ cutting circle $P E$ in d and produce $l q$ to meet $E E'$ in L . About L as centre strike arc $d h h'$ cutting $p n$ in h , and on this arc measure from h arc $h D$ equal to δ . Then $D r$ perpendicular to $d q L$ gives r the end of polar axis of Mars. Regarding $P E P'$ as disc of Mars, and $P P'$ as the meridian, while the minor semi-axis of the Martian equator at epoch is equal to $D r$. The part of this construction up to the point where the arc $p l$ is taken equal to $(\alpha - N)$ is common to all cases, and the construction from that point takes less time than is required for writing the description of it.

Mr. Proctor said he had another paper which was in fact an answer to a short paper by Mr. Stone on the question of the intrinsic brightness of objects seen with the telescope. It would perhaps be more interesting to the Fellows than the paper he had just read. The subject of intrinsic lustre was often misunderstood. The point which he had dwelt upon was this: if you have any surface whatever—the surface of a nebula for example, or part of the sun or a planet, or any object subtending a sensible angle and you form an image of this surface, whether magnified or not, it will always be of less intrinsic brightness than the real surface itself, or under very favourable conditions, of equal brightness, but it can never be of greater brightness. Mr. Stone said that this was demonstrated by Sir George Airy, but so far as he could judge from Mr. Stone's remarks, the original paper only referred

to refraction, and proved that a refracting medium could not cause a surface to appear brighter than itself. My paper pointed out that reflection also could not produce an increase of brightness. Mr. Stone went on to say (if I have not misunderstood him) that in star clusters the atmospheres of the suns might overlap. I concluded that he meant optically overlapping, not physical overlapping. In this I misunderstood him. But in return Mr. Stone has misunderstood me, because he seems to think that I had imagined that one could not, by telescopic power, increase the quantity of light got from an object. Mr. Stone asked, if that cannot be done, how is it that the satellites of Mars are visible with a telescope of 26 inches and not with a telescope of 4 inches. In reality the point I raised related only to surfaces. In the case of the satellites of Mars we do not see a surface, and in regard to the quantity of light received by the eye the aperture is all-important; but when it is a question of surface, although the visibility of an object may be greatly increased by the use of a telescope, the brightness of the surface cannot be so increased. I may mention (though it is not in the paper), the following illustration of the mistake people often make on this point. In the dusk of evening you take up a piece of printed paper, you find you can hardly read it, but when you look at it through a magnifying glass you can read it distinctly: now, many think that it must be a great deal brighter when the letters are thus seen; but look at both surfaces together and you will find the magnified surface is a great deal duller, though, being magnified, you can read it more easily.

Mr. Neison said he thought that Mr. Proctor had not fully caught the meaning of Mr. Stone. He thought that Mr. Stone intended to say that there was an additional reason for seeing small points of light with a large aperture over and above the mere increase in light collected by the object-glass, for with a large aperture you would have the light of the star collected into a smaller stellar disc, while with a smaller aperture it would be diffused over a larger disc, which would not be so easily seen in contrast with the illumination of the atmosphere. With regard to Sir George Airy's paper, Mr. Neison believed that Sir George showed that under no circumstances could you, as Mr. Proctor said, make any given illuminated area, either by transmitted light or reflected light, brighter than it really was.

Mr. Proctor: Does he actually state that general case? In that case he has thoroughly anticipated what I have said.

Mr. Neison: I do not know whether he included reflection explicitly, by he includes it implicitly when he deals with the question of light falling from the moon being reflected on the earth; but I must say that Mr. Proctor was the first to show me

that you cannot increase the brightness of an object by instrumental means.

Mr. Ranyard: The Astronomer-Royal's paper has reference to the bright light seen along the edge of the moon when the sun is partially eclipsed. The edge of the moon—that is, the part of the photosphere adjacent to the dark limb of the moon appears brighter. An idea was thrown out, I think by Professor Challis, that it might be caused by the atmosphere of the moon, and the Astronomer-Royal shows, as far as I remember, that no refraction through such an atmosphere could compress the light as it were together and increase the brightness of the background, so that it must be some effect of contrast or some physical effect.

Mr. Proctor: I must have read the Astronomer-Royal's paper, for I read Professor Challis's paper. I remember there were one or two errors in the latter. It must have been by an act of unconscious memory that I reproduced Sir G. Airy's arguments without knowing it.*

The thanks of the Society were given to Mr. Proctor for his paper.

Mr. Ranyard read a paper by Mr. Plummer *On the supposed influence of a mass of brickwork upon the errors of a transit instrument in its neighbourhood*. Mr. Plummer had found upon tabulating the azimuth errors of his instrument that they were positive in summer and negative during the winter months, and he concluded that these errors were due to the radiation of heat from a great mass of brickwork in the neighbourhood of the instrument, which formed the pier upon which the equatorial was supported.

The President said he could quite understand a large pier of brickwork absorbing, and afterwards giving out, heat so as to considerably affect the errors of a transit instrument in proximity to it. In the course of a visit which he paid last year to the Royal Observatory, Edinburgh, Prof. Piazzi Smyth had shown him an experiment, in order to prove the extreme sensitiveness of the piers of his transit instrument to slight changes of temperature. The telescope was turned to the nadir, and the images of the wires reflected from the mercury with a Bohnenberger eyepiece were brought into coincidence with the wires in the eyepiece. The light of a lantern, at a distance of some six feet, was then turned upon one of the piers, and in the course of

* [Note by Mr. Proctor.] I have since noticed that the papers are in different volumes of the *Monthly Notices*. Prof. Challis's in Vol. XXII., which I possess, Sir G. Airy's in Vol. XXIV., of which I have not a copy. I note also that Mr. Neison's interpretation of Mr. Stone's meaning does not appear to be supported by anything in Mr. Stone's paper.

three or four minutes the heat of the lantern caused the pier to buckle, so that the images of the wires were carried distinctly away from coincidence.

Mr. Dunkin : Perhaps I may be permitted to make a remark on this subject. Mr. Plummer's discovery of a periodic change in the instrumental errors of his transit instrument is nothing extraordinary. Such changes occur in all transit instruments. I may mention that in the transit circle at the Royal Observatory, a periodical change has been noticed annually since its erection in 1850. These changes are so constant, that I could almost tell you from month to month what the level error or azimuthal error ought to be to the tenth part of a second. The origin of it is undiscovered to the present moment. Whether Greenwich Hill shifts periodically, owing to the difference of temperature in winter and summer—which is possible—or whether from the same cause the two piers are subject to an unequal expansion, I do not know, still the fact that such changes take place in the instrumental errors of all transit instruments is well known. Looking over Mr. Plummer's paper this afternoon, it appeared to me very doubtful whether the brickwork pier is the cause; it may be that the ground itself is shifting, as it is suspected at Greenwich, or that there is some other cause. If the sun shines on the brickwork of the transit pier to any extent in summer, that may be the cause of the changes, but it is nothing new to find that transit instruments are subject to these periodical variations in the errors of level and azimuth.

Mr. Talmage referred to a discussion at the Royal Institute of British Architects on Col. Tomline's observatory, which was then said to be close to the heating apparatus of a Turkish bath. He observed that Mr. Penrose was at that meeting, and perhaps he might be able to say something on the subject.

Mr. Ranyard said he thought that the changes would not be due to the Turkish bath, as the azimuth errors were positive in summer and negative in winter.

Mr. Penrose said in addition to the discussion at the Institute of British Architects, which had been referred to, he might mention another fact which bore on the subject. At St. Paul's Cathedral there was a very curious arrangement for sliding a rail across the west front at night. It slides up in a groove three or four inches wide, on the landing of the broad mass of steps. In hot weather that groove was so much contracted that there was great difficulty in getting the rail to pass through. He had had occasion to measure the groove, and there was proof that there was fully an eighth of an inch movement between the granite steps and the marble slabs which form the platform.

Mr. Gill mentioned a curious case that had come under his notice, in relation to the Liverpool Observatory, which was built down by the water, near to the docks. There was a regular change there in the errors, which was found to be influenced by the height of the tide, which passed under or close by the transit instrument. The change of the azimuth and level was proportioned to the height of the tide. Another such instance occurred in the case of the transit instrument at Rodrigue, in connection with the transit of Venus. In that case there was a curious diurnal change of level from the maximum in the daytime to the minimum at midnight. The difference amounted to about three or four seconds of arc. At Poulkova they attached great importance to the symmetrical arrangement of the building with reference to the piers of the transit instrument, and they thought that if they had another first-class observatory to build they would take care that all the surroundings of the observatory were symmetrical with the instrument, in order that radiation from every direction might be equal. A suggestion, however, has been made that the piers might be surrounded by a coil of pipes with water from the ordinary supply, which continually traversing the pipes would preserve a uniform temperature. This would be perfectly practicable, provided the water company did not charge too much for the supply. (Laughter.)

Mr. Christie said as the tide at Liverpool had been referred to, it might be interesting to know that Mr. Stone had established, as he believed, a connection between the changes of the azimuth errors of the transit instrument, at the Cape Observatory, and the amount of water in the river at places near it.

The President: That perhaps may be accounted for by the percolation of the water through the soil slightly moving the foundation.

Mr. Christie: I do not attempt to account for it.

Mr. Dunkin: The Greenwich Observatory is built on very dry gravel soil, and there is no water there.

Mr. Neison: Another case is perhaps quite as marvellous. It is the case of the observatory at Armagh. The observatory there is on a hill, and the geological foundation on which it stands consists of sloping strata of clay. During the winter months the rain comes down, the clay gets saturated, and the effect is that the whole mountain is tilted up, whilst in the summer, when the clay gets dry, the mountain falls back.

Capt. Noble: In my own transit instrument I have both of these changes combined. My observatory is built on six brick piers, raised from the ground to allow of ventilation. The transit pier is 4-ft. square at the ground, lessening to 2-ft. by 18-in.,

and it goes 6-ft. below the ground in a bed of very thick clay. If I were to go at any time into my observatory blindfold, I could tell the time of day to within two or three hours, from the transit level. The east end of the level goes up in the morning and gradually descends until the evening, when it gets to its normal position. If the transit were carefully levelled to-night at 7 o'clock, I should know, if the weather remained the same, that it would be within a fraction of a second of arc at the same place at the same hour to-morrow night, but at 8 a.m. I should find it had gone up towards the east. This end of the axis also rises invariably in wet weather. I never can tell why the eastern side of the pier rises so much under these circumstances. It shows how very sensitive the level is. As to the absorption of heat, my brick-work is always kept whitewashed a pure white, in order to reflect as much of the solar rays as possible.

Mr. Dunkin: Do you mean to say the sun is allowed to shine on the pier? because, if so, I do not wonder at the change of level.

Mr. Christie: One can easily understand how the level error changes, but the curious thing is how the ground or piers should shift so as to cause the azimuth error to change. The azimuth error seems to follow a law just as definite as that which the level error follows, though the epoch is not the same. That is the curious part, as it seems to me.

The thanks of the Society were voted to Mr. Plummer for his paper.

Mr. Ranyard read a paper by Mr. Stone *On the telescopic observations of the transit of Venus*. Mr. Stone had rediscussed the observations published in the parliamentary report upon the observations of the transit of Venus, and had deduced a solar parallax of $8''.89$ —equivalent to a sun's distance of 91,940,000 miles.

Mr. Ranyard mentioned that since the publication of the parliamentary report, Captain Tupman had made a new discussion of the observations, and had arrived at a larger value—which he understood was almost identical with the value given by Mr. Stone.

Mr. Christie said that he thought that there must be some misunderstanding. He understood Capt. Tupman to have mentioned the matter at the Council in joke as an instance of what might be done by rejecting observations, provided you only take such observations as suit your purpose.

Mr. Ranyard: I understood him to mean he had used a different method.

Mr. Christie: Oh, no.

Mr. Ranyard: Captain Tupman will, I understand, publish a short note on the subject in the monthly notices, and will explain himself. This paper will also be published so that the Fellows will have the opportunity of judging of the different methods of selecting observations that have been employed by Captain Tupman and Mr. Stone.

Mr. Gill said there were many points in Mr. Stone's paper of extreme interest. Mr. Stone had produced a result very different from that of the Astronomer-Royal and Captain Tupman. He thought Mr. Stone was wrong in his remarks about the observations at Honolulu, because the diagrams showed clearly that more than one phase had been observed. He had looked through the observations of a number of these contacts and had communicated his opinions on them to the Astronomer-Royal without knowing what effect any alteration of the contacts would have. There appear to be 4 distinct classes of phases of contact in the transit of Venus observations; and there were possible physical explanations of them all, if they supposed the sun had a true diameter, not increased by irradiation, and another apparent diameter, increased by irradiation; and if they also supposed that Venus presented a black disc, and besides that also presented a certain increased "something" which was due to atmosphere. Then the moment the atmosphere of Venus came out to the true edge of the sun the effect would probably be to produce an interference of some kind, first a grayness or blackness, a shade or change of light, and as this gets further over, the atmosphere overlaps the apparent disc of the sun, and another apparent change takes place; and, thirdly, comes the time when the true diameter, or black diameter, of Venus touched the true edge of the sun, producing what was supposed to be the phenomenon of the black drop; and finally it arrived at the fourth stage, which was the black limb of Venus forming a geometrical contact with the sun, and the whole was completed when a ring of light showed itself round Venus. It was worthy of remark that some observers called the same thing black and others white (laughter), but all depended on the kind of shade they had and the kind of eyepiece used, and on many other things. It was a difficult problem, but he could not see how so very large an angular parallax had been arrived at by Mr. Stone, the discordance being nearly a million of miles, as compared with the results Captain Tupman and the Astronomer-Royal had arrived at. If one constant in astronomy was well determined, probably it was Struve's constant of aberration. And if that constant was correct, then combined with Cornu's determination of the velocity of light, it would give 8.78 for the solar parallax and not 8.86. I think at least that

Struve's determination is the best, and it is unfair to quote any other combined with Cornu's determination of light velocity in preference to Struve's, as a proof that the parallax must be about 8".86. He would be glad to hear the observations of gentlemen who were smilingly objecting to Struve's determination.

Mr. Christie pleaded guilty to having smilingly objected, but he did so solely on the ground that there was a discrepancy between the determinations of the sun's distance from the constant of aberration and from the eclipses of Jupiter's satellites, and therefore he thought it not desirable to commit himself. He did not wish to throw any doubt on Struve's co-efficient, because it would give a parallax he should be inclined to accept.

Mr. Neison: Mr. Gill rather startled me by saying he preferred Struve's constant of aberration. If I remember rightly, Struve's value is obtained from stellar observations. I think Mr. Gill had a strong objection on principle to any value of the constant of aberration derived from observations of the stars, without taking into consideration the motions of the sun in space, upon the ground that as we do not know what the motion of the sun is in space we may get an erroneous value, unless we take our stars at a proper distance on each side of the apparent path of the sun, which, as far as I know, is not a point which Struve paid any attention to. We may thus have an unknown error.

Mr. Gill: I quite agree with Mr. Neison as to the desirability of another determination of the constant of aberration in the southern hemisphere; but of the determinations which we have, I only contend that Struve's is best.

The meeting adjourned at a quarter to ten o'clock.

REVIEWS.

A Treatise on the Cycloid and all forms of Cycloidal Curves, and on the use of such Curves in dealing with the motion of Planets, Comets, &c., and of matter projected from the Sun. By Richard A. Proctor, B.A., &c. With 161 Illustrations and many examples. For the use of students in universities, &c. London: Longmans. 1878.

Encore Proctor! Toujours Proctor! Here he is again, with what we imagine is the most exhaustive treatise on these curves that was ever written. The contents are: Section I. The right cycloid. II. The epicycloid and hypocycloid, with appendix. III. Trochoids, with appendix. IV. Motion in cycloidal curves. V. Epicyclies, with appendix. VI. Equations to cycloidal curves. VII. Graphical use of cycloidal curves to determine (1) The motions of planets and comets, (2) The motion of matter projected from the sun. Examples. How in the world Mr. Proctor manages to find time to bring out so many works of high merit is a puzzle to us. It is like the facile copiousness of some of our modern novelists. But his subjects are not such as to be dashed off *stans pede in uno*. Not having studied these curves beyond the examples of them occurring in works on the calculus, we have been surprised at the

success with which their properties are here investigated geometrically. Our mathematical readers cannot fail to appreciate this remarkable treatise, which will be a rich treat to those who delight in such studies for their own sake as well as for their utility. They will welcome the guidance of a master who is as much at home amidst these intricacies as was Theseus in the Cretan labyrinth with the clue of Ariadne. Some of the illustrations are due to Mr. H. Perigal, and are mechanically drawn by the lathe. Of these are the elegant figures of the epicycles traversed by the planets (approximately) with reference to the earth regarded as fixed. We may advert again to this work at another time when we shall have had leisure to give more attention to the latter part of it. So far as we have carefully examined it, the *errata* seem very few. We have noted the following: page 9, line 14, triangles are $P G A'$ and $P A' B'$; page 30, line 7, the figure 8 is omitted, do., line 9 from bottom, *shred*, *Q. shell?* page 32, line 4, angle $A D B$; page 33, line 3, Prop. XIII.; page 46, lines 9, 11 (twice) F should be in the denominator instead of R ; do., lines 16, 17, same; page 48, line 14, $\frac{O M}{O B}$ arc $B Q$ is omitted; page 49, line 3 from bottom, Ck should be CK ; page 50, line 4, Prop. III.; page 54, line 17, rP should be rR ; page 58, line 12, $O A a$ should be $O A E$; page 59, line 3 from bottom, $P' a t$ should be $P' a T$, and $P' t = A' T$ should be $P' T = A' t(?)$; page 64, line 7, $A P B$ should be $A P D$; page 68, line 7, $B O p$ should be $B O b$; page 69, line 8 from bottom, $A P B$ should be $A P D$; page 71, line 5, $A P B$ should be $A P D$; page 73, line 8, $p B$ should be $b B$; page 75, line 18, $B b$ should be $k b$; page 78, 4 from bottom, $P' n$ should be $P' m$; page 79, 6 from bottom, $A P b$ should be $a P b$; page 85, line 6, N should be N' ; do., 5 from bottom, $O D$ should be $B D$; page 87, lines 8 and 12 at end of each, within the brackets, θ is omitted.

Other Worlds than Ours: the Plurality of Worlds studied under the light of recent scientific researches. By Richard A. Proctor. Fourth edition. Longmans. 1878.

Though partially acquainted from other sources with the author's views on the subject here treated, the present work had never come into our hands. It was then with no little pleasure that we read this fourth edition, and found that it exceeded our expectations. To such as have not read it we commend it as full of interest and instruction. They will find the fascinating subject treated with sobriety, good judgment, and comprehensiveness, and illumined with the latest lights of astronomical science and discovery. The illustrations are beautiful. Between the views of those who see worlds actually and everywhere, and of those who think our own planet is the only one, there is a *via media*, which is likely to be followed by the generality of such as have been at the pains to master all the many details of the question. Young's familiar line came to our recollection after perusing this excellent work, and it intensified our appreciation of the wisdom, knowledge, and power displayed in the absolutely much, though comparatively little, we are enabled to discern of the "wondrous works of Him who is perfect in knowledge."

The Unseen Universe, or Physical Speculations on a Future State. By B. Stewart and P. G. Tait. Sixth edition (revised and enlarged). London: Macmillan & Co. 1876. Price 6s.

The following are extracts from the prefaces. "We are in hopes that when this region of thought comes to be further examined, it may lead to some common ground on which followers of science on the one hand,

and of revealed religion on the other, may meet together and recognise each other's claims without any sacrifice of the spirit of independence, or any diminution of self-respect. Entertaining these views, we shall welcome with sincere pleasure any remarks or criticism on these speculations of ours, whether by the leaders of scientific thought, or by those of religious inquiry." "Our object . . . is to endeavour to show that the presumed incompatibility of science and religion does not exist. This, indeed, ought to be self-evident to all who believe that the Creator of the Universe is Himself the author of Revelation. But it is strangely impressive to note how very little often suffices to alarm even the firmest of human faith." "*From a purely physical point of view, we contend for the possibility of immortality and of a personal God.*" "We have merely stripped off the hideous mask with which materialism has covered the face of nature to find underneath (what every one with faith in anything at all must expect to find) something of surpassing beauty, but yet of inscrutable depth. For, indeed, we are entire believers in the infinite depth of nature, and hold that just as we must imagine space and duration to be infinite, so must we imagine the structural complexity of the universe to be infinite also. To our minds it appears no less false to pronounce eternal *that aggregation we call the atom*, than it would be to pronounce eternal *that aggregation we call the sun*." "We do not pretend to be theologians in any sense of the word. . . . We are, no doubt, endeavouring to bring about a reconciliation between science and religion. In order to accomplish this we must first find out what is the fundamental principle of science, next what is the fundamental creed of the great majority of Christians, and then endeavour to show that the two are not incompatible with each other. In carrying out this process we have been led to regard the Principle of Continuity as the great law which regulates scientific inquiry, and there cannot be a doubt that the Old and New Testaments are regarded as authoritative expositions of religious truth by the great majority of the Church of Christ. Now we find that the expressions in the Scriptures regarding the future of man and the constitution of the unseen world, taken in their obvious, if not absolutely literal meaning, are not inconsistent with scientific deductions from the Principle of Continuity."

We also extract the following from the introduction:—"We attempt to show that we are absolutely driven by scientific principles to acknowledge the existence of an Unseen Universe, and by scientific analogy to conclude that it is full of life and intelligence—that it is, in fact, a spiritual universe and not a dead one." "We are likewise led to regard the Unseen as having given birth to the present universe, a conclusion to which one of our leading critics has apparently given his assent." "We maintain that the visible universe—that is to say, the universe of atoms—must have had its origin in time, and while THE UNIVERSE is, in its widest sense, alike eternal and infinite, the universe of atoms cannot certainly have existed from all eternity."

"We come now to the second part of our subject. All that we have yet endeavoured to show is that the theory of a future life is not in any way whatever in contradiction to any ascertained facts or principles of science. But we have not succeeded in finding any proof that arrangements have been made in the Unseen world for our translation thither after death. It has been shown that there is nothing in the whole range of science to lead us to suppose that life is impossible after death; but we have yet to enquire what evidence, if any, exists in favour of a future state. Now, it is well-known that the followers of Christianity believe they have received such evidence in virtue of the resurrection of Christ

and it is equally well-known that of late years a school of scientific men has arisen who regard such an event as one impossible to be believed. . . Now we believe that an extension of purely scientific logic drives us to receive as quite certain the occurrence of two events which are as incomprehensible as any miracle; these are—the introduction of visible matter and its energy, and of visible living things into the universe. Furthermore, we are led by scientific analogy to regard the agency in virtue of which these two astounding events were brought about as an intelligent agency, an agency whose choice of the time for action is determined by considerations similar in their nature to those which influence a human being when he chooses the proper moment for the accomplishing of his purpose. If this be true, the discussion regarding miracles must be removed altogether from the domain of science, and this for the very good reason that scientific logic admits the occurrence of events at least as astounding. The question is now rather one for the historian and the moral philosopher to decide. The first of these is clearly bound to examine the evidence in favour of the life and resurrection of Christ, while the latter is bound to look around and ask what moral necessity there was for the interference of this peculiar intelligent agency, and also whether, as a matter of fact, the interference has proved beneficial. But neither of these two ways of regarding the subject is at all cognate to our enquiry. We simply show that a reception of the miracles of Christ leads to no intellectual confusion." "The statements in the New Testament scriptures regarding God are necessarily mysterious, but mystery can be no test of their truth or falsehood, inasmuch as it must in such regions be the almost inevitable accompaniment of truth. The question is not whether they are mysterious, but whether they are consistent with themselves and with the knowledge we derive from other sources. We therefore devote considerable portions of this volume to a proof that the conception of God which the majority of Christians derive from the New Testament is in no way inconsistent with that deduced from scientific principles." "We have tried honestly to view things with two eyes—the eye of knowledge and the eye of faith; first with one, then with the other, finally with both. To what extent we have succeeded is, after all, a matter of minor importance if only the lawfulness of this mode of vision be ultimately allowed. And just as we have a better appreciation of the form and distance of natural objects when we view them with both our physical eyes, so, we venture to think, must it prove with the truths of which we now speak. We have explained that the first part of our argument is altogether independent of revelation; proceeding, as it does, solely upon scientific data, and the conclusions which these seem to render inevitable. In the second part, however, we feel that we ought not to deprive ourselves of the overwhelming additional evidence which we derive from Christian records. Here, therefore, we shall neither gratify one class of our critics by starting from a point which ignores what we regard as the fully warranted belief of the great majority of Christians, nor shall we be overruled by the excessive timidity of another class who apparently regard a two-eyed man as a monster in those regions where truths of really vital importance are concerned."

These extracts, though with the disadvantage of being taken out of their context, will afford an idea of this remarkable and able work. A sketch is prefixed, interesting, though avowedly imperfect, of the various beliefs in a future existence held by the different branches of the human race; ancient Egyptians, Hebrews, Greeks and Romans, Eastern Aryans, Christians, Mohammedans, Swedenborgians, and modern spiritualists, &c.

Ch. II., *Position taken by the authors.—Physical axioms.* Here the Principle of Continuity is explained and variously applied; and the sound conclusion is reached, that "continuity does not preclude the occurrence of strange, abrupt, unforeseen events in the history of the universe, but only of such events as must finally and for ever put to confusion the intelligent beings who regard them." (p. 88.) The authors point out similar errors committed by the extreme schools of theology and science. We were reminded in this and other parts of the work of some of the excellent remarks of the Duke of Argyll in his "*Reign of Law*," ch. I., &c., especially regarding the close connection between religion and science (pp. 57, 58, 5th ed.). Concerning the universe the writers remark,—"We do not hesitate to assert that the visible universe cannot comprehend the whole works of God, because it had its beginning in time, and will also come to an end. Perhaps, indeed, it forms only an infinitesimal portion of that stupendous whole which is alone entitled to be called THE UNIVERSE." (p. 96.) Similarly does Isaac Taylor express himself in his deeply interesting book on the *Physical Theory of Another Life*. (p. 317.) "Let every one then—every one capable of holding correspondence with the Creative Mind, . . . read in the visible heavens, a dim, and yet not fallacious presage of the vastness, and depth, and height, of that unseen economy, with which he shall find his destinies involved; and let him believe that, when this now unseen economy comes to be known, the vastness of the material theatre shall cease to attract regard, in comparison with the stupendous movements and destinies which it sustains."

Cb. III. *The Present Physical Universe*, amongst other things treats of *Energy*, its theory and transformations, and final degradation. "It is absolutely certain that life, so far as it is physical, depends essentially upon transformations of energy; it is also absolutely certain that age after age the possibility of such transformations is becoming less and less; and, so far as we yet know, the final state of the present universe must be an aggregation (into one mass) of all the matter it contains . . . But the present potential energy of the solar system is so enormous . . . that it may supply for absolutely incalculable future ages what is required for the physical existence of life. Again, the fall together, from the distance of Sirius, let us say, of the sun and an equal star would at once supply the sun with at least as much energy for future radiation to possible planets as could possibly have been acquired by his own materials in their original falling together from practically infinite diffusion, as a cloud of stones or dust, or a nebula; so that it is certain that, if the present physical laws remain long enough in operation, there will be (at immense intervals of time) mighty catastrophes, due to the crashing together of defunct suns—the smashing of the greater part of each into nebulous dust surrounding the remainder, which will form an intensely heated nucleus—then, possibly, the formation of a new and larger set of planets with a proportionately larger and hotter sun, a solar system on a far grander scale than the present, and so on, growing in grandeur but diminishing in number till the exhaustion of energy is complete, and after that eternal rest, so far at least as visible motion is concerned." "The study of the necessary future has prepared us for an inquiry into the long remote past. Just as the present discrete stellar systems must finally come together, so the materials which now form them must have originally been widely separate. Our modern knowledge enables us to look back with almost certitude to the time when there was nothing but gravitating matter and its potential energy throughout the expanse of space—ready, as slight local differences of distribution predisposed it, to break up into portions, each converging to one or more

nuclei of its own, and thus forming in time separate solar or stellar systems. We have thus reached the beginning as well as the end of the present visible universe, and have come to the conclusion that it began in time and will in time come to an end. Immortality is, therefore, impossible in such a universe." (pp. 127, 128.)

The Transactions of the Royal Irish Academy, Vol. XXVI. Science. VII.

The Red Stars: Observations and Catalogue. By J. Birmingham.

Dublin, and Williams and Norgate, London (105 pp. 4to, price 4s.)

This paper was read June 26th, 1876. Schjellerup's catalogue is here greatly augmented. We have the names, position, &c., of 658 red stars, then addenda of 65 more, and a supplemental list of red and deep yellow stars from Struve and Sir J. Herschel, comprising about 400. This, the fullest catalogue in existence of red stars, with introduction, copious and valuable annotations, and spectroscopic observations, will no doubt find its way into the hands of many observers, who will not fail to appreciate the pains with which it has been prepared. It is indebted, besides those already named, to Webb, Schmidt, Burton of the Dublin Observatory, Secchi, and others. The author's own observations were made with a 4½-in. telescope, and power of 53, with which the colours of stars could be well defined down to the 10th mag. Mr. Birmingham quotes Schmidt's remark that "no stars have been found of a perfectly red, or blood colour, such as may be seen in the solar protuberances; even stars like the 'crimson' in Lepus, or Herschel's 'garnet star' are no exceptions, and the reddest star that we see still shows a mixture of yellow," and says that this perfectly agrees with his own observations. His opinion of the cause of variability of stars is that it is owing to the intervention and recession of nebulous matter. "May not," he asks, "the yellow spectral type of our own sun be caused by a nebulous intervention, and might not the many historical obfuscations of his light, with various durations, have been due to nebulous trains of greater or less length moving in non-re-entering courses? Of such phenomena we ourselves may remember a striking example that appeared with a cloudless sky in the west of Ireland on May 22nd, 1870, in Greenwich on the 23rd, and on the same day, but at a later hour, in north-eastern France—a progressive manifestation that seems to accord well with the hypothesis of moving nebulous matter." We refrain from making more extracts, as the moderate price of the work puts it within the reach of all. Our best thanks are due to its able and careful author.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

JUPITER'S ATMOSPHERE.

Sir,—Mr. Proctor's letter in your last number appears to me to need little more than a mere acknowledgment on my part. It pleases Mr. Proctor to be paradoxical, and to assert that the well-known physical

LAT = 50°

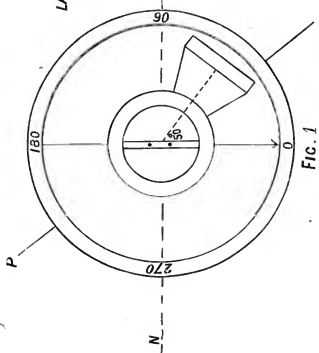


FIG. 1

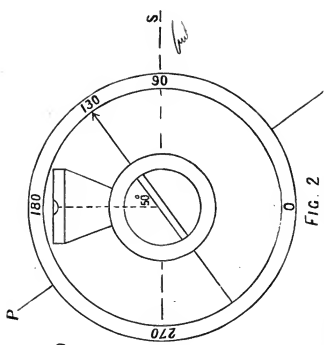


FIG. 2

COMBINED POSITION AND SETTING CIRCLE,
 OBLIVATING THE NEED OF A DECLINATION CIRCLE ON NEWTONIAN EQUATORIALS.
 [See Page 103.]

laws connecting the density, pressure, volume, and temperature of a gas are empirical to such an extent that they cannot be trusted. These laws are less empirical than the theory of gravity, and like it have been found to be true under every condition under which they have been experimentally tested, although, like in the theory of gravity, there are minute variations which have only in part been successfully explained. They have been shown to be a necessary sequence to the well-established dynamical theory of gases, and they have been experimentally verified by many physicists for far wider ranges than are necessary to establish the conclusions in my investigations. I do not think, therefore, that I can be called on to combat every paradoxical notion of Mr. Proctor, or to substantiate the truth of such generally received laws which no one could think of doubting but Mr. Proctor. Possibly Mr. Proctor may assert next that this is not so; it is, however, a matter of fact which does not admit of discussion.

I have no objection to Mr. Proctor asserting that my investigations are unsound, if that course should please him. My own investigations are perfectly in accordance with those of the many distinguished investigators in the same field, so that all must stand or fall together, and had he thought fit to read their memoirs, he would have found many of his objections already met and disposed of. Mr. Proctor is, of course, entitled to maintain, as often as he pleases, that he alone is in the right; but I would submit that when he attacks laws forming the very basis of this branch of physical science, before accepting his isolated opinion, we are entitled to demand something more in its support than simple assertion, or superficial popular reasoning.

With regard to the personal portions of Mr. Proctor's letter, I would prefer to say nothing, and I think, Sir, you will find my letter in your February number is free from anything of this kind. I must decline to be held responsible when Mr. Proctor's prejudices lead him to twist portions of courteously worded communications into personal allusions to his own supposed shortcomings. I am content to accept Mr. Proctor's assurance about the very great facility with which he does original work, so that he finds it a loss of time to read the mathematical investigations of others, even though I might wish for the sake of science that it were otherwise. There are unfortunately few who can thus claim to be able to do better themselves than any of their predecessors. It would appear to be useless to assure Mr. Proctor that my own results rest on a perfectly independent investigation. Further, I must admit that Mr. Proctor is right on one point, inasmuch as for the purpose of being more thoroughly acquainted with my subject, before undertaking further investigation in any direction, I do think it desirable to be familiar with the work of my predecessors in the same field, and where possible I like to consult the original memoirs, as well as gather any information from the occasionally inaccurate popular text-books.

March 4, 1878.

E. NEISON.

COMBINED POSITION AND SETTING CIRCLE,

OBVIATING THE NEED OF A DECLINATION CIRCLE ON NEWTONIAN
EQUATORIALS.

(See Frontispiece.)

If you take an ordinary position circle, in which the circle remains fixed while the eye-piece rotates, and attach a bubble to the vernier so that the angle, included between the diameter indicated by the bubble and the

diameter indicated by the lines in the eye-piece, shall be equal to the latitude of the place, and if the apparatus be fixed on the west side of the telescope it will be found that whereas the vernier set by the eye-piece lines on a double star gives the position of the components in the usual way; the same vernier, set by the bubble, will give the Z D of the telescope; and accordingly, if the telescope be turned into the meridian the same vernier and circle that give the position can also be made to give the N. P. D. of the object to which the telescope is directed.

If the position circle, figured in the usual way, were placed on the east side of the telescope, the quantities read by the bubble would be S. P. D.

Let N. S. represent the axis of the telescope, seen from the west, directed to an object in the meridian and on the horizon.

Then Fig. 1 gives the position of a double star = $0-180$, while Fig. 2 gives the polar distance of that star = 130° .

W. ERCK.

DOUBLE STAR Σ 951 (TRIPLE).

Sir,—The position of this double star, according to Lord Lindsay's admirable summary of Struve's double stars, brought down to the epoch 1875.0, is R.A. 6h. 33m. 25s.; D. $+9^\circ 55'$; Magnitudes, 8.5 and 10.7; Position, $308^\circ 96'$; Distance, $21'' 35$ (1830.7).

On the evening of the 3rd instant, I made a hasty examination of this object, with 4.3 inch Wray achromatic, and found that Struve's 10.7 mag. comes (B) is really double, as there is a fainter comes (C) near to it; P. $200^\circ \pm$; D. $7'' \pm$ from B, but these are merely approximate estimations, as haze prevented further observation. A large aperture will be required to measure it.

Belfast: February 12, 1877.

Yours faithfully,

ISAAC W. WARD.

η GEMINORUM.

(Translated from *Astronomische Nachrichten*, No. 2174.)
(Letter from Mr. Torvald Köhl to the Editor of the *A. N.*)

On directing my telescope of 3-in. aperture and 45 inches focal length to the constellation Gemini at 10 o'clock yesterday evening, the 29th of December, I was struck by the quite red appearance of the star η , even when looked at with a power of only 42. With a power of 168, one which usually brings out the colour of the stars exceptionally well, the colour of the star in question could be compared only with that of α Ceti.

In order to arrive at a correct judgment, I subsequently directed the telescope towards several of the adjacent stars, and saw that μ , too, showed a reddish lustre, whereas all the other stars presented the usual appearance.

On the same evening α Ursæ Majoris, a star which is also variable in colour, was reddish.

In Professor Schjellerup's "Catalogue of isolated red stars, completed and continued up till the end 1874," the stars above named are not included, although at present η Geminorum is red and μ Geminorum deep orange.

Copenhagen: Dec. 30, 1877.

TORVALD KOHL.

P.S.—Since the above was translated, Mr. Birmingham (with reference to what Mr. Köhl says respecting the colour of η Geminorum) observes (*A. N.* No. 2178) that this star, and also μ Geminorum are included in his

Catalogue of red stars published in the *Transactions of the Royal Irish Academy*, August, 1877, of which it forms Part 7 of Vol. XXVI. This Part is to be had separately.

SATELLITES OF JUPITER AND SATURN.

Sir,—If Mr. Marth or some astronomer would be so kind as to furnish the longitudes of Jupiter's and Saturn's satellites, for Dec. 31st, ob. 1876 and 1878, or for Dec. 31st, ob. for common, and Jan. 1st, ob. for leap years, then the tables in *Vince's Astronomy* might be used. The ease and simplicity with which the positions could be obtained by this method would be of great use and importance to a great many observers. It is true, for England, the *Nautical Almanac* in a great measure supplies all the configuration. In America their nautical almanac gives the co-ordinates of the satellites by which the exact position of the 1st, 2nd, and 3rd may be computed. But for the 4th satellite the co-ordinates are only given about the time the 4th satellite will be eclipsed. Hence at other times the 4th satellite moving among the other three make it at times quite hard to distinguish it from the others. The longitudes of the satellites could be readily computed from knowing the exact time of conjunction of each satellite with the planet. But all the particulars in regards to finding the longitudes are clearly explained in Vol. I. *Vince's Astronomy*. The above tables would project all the satellites of Jupiter in a right line. This would be sufficient to distinguish the satellites from one another. But if a nearer result is desired, it could easily be obtained by drawing through Jupiter lines making angles equal to the inclination of the satellites, and on these laying off the distances given by the tables.

It seems to me that such tables as I have indicated would be of so much use and importance to all observers that it is certainly worth the little trouble necessary to compute the longitudes. Besides, a great many already have the tables, and only require the longitudes of the satellites.

JAMES H. GARDINER.

United States: Newhargh, N.S.

THE RING OF LINNÉ.

Sir,—I must admit that when I wrote the letter which appears on your p. 125, I thought that E. Neison intended to imply that Linné had altered its appearance since 1869, but I now see that that was not intended.

Nevertheless, it is difficult to believe that what appears in my 4½-inch refractor as a perfectly round, though faint, ring, is not a true crater-formation, but, as E. Neison thinks, is only "a number of ridges and mounds." This is especially the case when it is remembered that the bright spot is on the W. side of the ring at sunrise, but on the E. side at sunset, and that the ring is entirely included within the white spot which marks Linné when the sun is high upon it.

At sunrise this ring is best seen when the terminator is on the "strait" between the Apennines and Caucasus, which joins Mare Serenitatis to Mare Imbrium. Comparing my various observations, I find it is only visible for about 9 hours, counting from the earliest time at which it becomes imperfectly visible to the latest time to which I have suspected it—i.e., from 2 hours before the terminator reaches the W. edge of the

above-mentioned "strait," to about 3 hours after it crosses the E. edge. At sunset the ring is visible when Bessel is on or beyond the terminator.

I estimate the diameter of the ring to be $\frac{1}{4}$ that of the largest of the craters to the N.W. of Linné (1 E. β^1 of the B. A. map).

It would appear that this is the same ring as that seen by Mädler on May 10, 1867 (see vol. VI., p. 241), and which he says appeared just the same as in 1831, when it was $6\frac{1}{2}$ English miles in diameter, and yet he says (vol. VI., p. 214) that at that time it still had an interior shadow when the sun had attained an altitude of 30° !

Capt. Nohle refers to the crater drawn by Carpenter in the frontispiece to your vol. VI. This cannot be the ring I allude to.

Sunderland: Aug. 17, 1877.

Yours truly,

T. W. BACKHOUSE.

"IS a CETI A RADIANT POINT?"

Sir,—Mr. E. F. Sawyer (*Register*, p. 124) asks for information as to the radiant point of five pretty bright meteors observed by him on the nights of October 17—20, 1876. The dates agree with the maximum of the Orionids; and no doubt they were either members of this shower or of another further south, in the same constellation. Several positions here for October are given in the catalogues of meteor observers. Thus Tupman found a shower near κ Orionis (October 5—14, 1869), and also near β Canis Majoris (October 11—12, 1869), Schmidt near ζ Orionis (October 18—27), and Greg gives a radiant near β Orionis for the entire month. There is also a shower (observed by Tupman on October 14, 1869), at R.A. 0, 44 Dec. 4° +, very close to a Ceti, but this can hardly have supplied the meteors seen by Mr. Sawyer (who has kindly furnished me with a diagram of their apparent tracks), as they began near that star and had comparatively long courses.

Mr. Sawyer mentions that his average number visible for the half-an-hour, 9 to 9.30 p.m. is 2.88. My hourly number, p.m., (mean about 9.30), is 7.51, deduced from 120 hours' watching during last fourteen months. Thus my half hour's average (3.75) is considerably above Mr. Sawyer's estimate.

I am, sir, your obedient servant,

W. F. DENNING.

Ashley Down, Bristol:

June 7, 1877.

MARS.

Mr. Brett's paper read before the Royal Astronomical Society at its November meeting is a singular production. He claims to have made his observations with a gin. "With" mirror. I cannot say that I made any course of observations on Mars, but I several times cursorily examined it with a 94 in. "With," and those observations point almost diametrically opposite to those of Mr. Brett. In the first place Mr. Brett says that the opacity of the atmosphere was shown by the great brightness of the disc at the limb; but I always found the centre (or the neighbourhood of the centre) of the disc very much brighter than the limb, that is, of course, disregarding the bright southern pole. Omitting some minor details we come to the south polar cap, this Mr. Brett considers must lie considerably above the surface of the planet, clouds in the upper region of the atmosphere, and he believes that he has observed their shadow cast upon the planet. When the snow-cap is upon the

limb, it certainly does present the illusion of appearing raised above the rest of the limb, but irradiation is the cause of this. There is a portion of the planet surrounding the snow-cap over the south pole which is much darker than the rest of the planet. This darkness is supposed to be partially natural from the dull blue of the marsy land immediately surrounding the snow, and partially an effect of contrast. It would be necessary for the clouds to be raised to a tremendous level to throw a shadow perceptible by us; what Mr. Brett saw was apparently the intense darkening bordering on the snow-cap. An 8 $\frac{1}{2}$ in. Culver mirror during last summer succeeded in showing Mr. Sadler a shadow spot on Jupiter broken up into filaments of light and shade.

10, Terminus Terrace,

F. C. DENNETT.

Southampton: Jan. 16.

URANUS.

Sir,—In the January number of this Journal, Mr. Marth calls attention to the visibility of Uranus to the naked eye, the present proximity to Regulus enabling one readily to direct the eye to the right spot. Within the last ten days, I have, on several occasions, easily seen it with the unassisted eye. Last night at 10h. 30m., though the moon was not set, I glimpsed it at the apex of an obtuse-angled scalene triangle, of which the line joining Regulus and ν Leonis forms the base. It has visibly moved since I first noticed it. With a very small low power binocular, it seemed to me decidedly fainter than ν Leonis, of the magnitude 5.6 of Heiss, and 5 of Argelander.

At about 11h. 30m., with powers 445, 550, on Cooke's 8-inch equatorial, (just to try the effect of magnifying on the disk) I glimpsed with certainty a most minute star, at, as nearly as I could estimate, an angle of 350°, and distance 24" (six diameters of Uranus). It seemed quite as faint as the distant comes to Bird's delicate double star following Procyon, and must have been nearly one of Struve's 12th magnitude. According to Mr. Marth's ephemeris in your last number, both Oberon and Titania were at their northern elongation on the 9th instant; could this speck be either of these satellites on their route to inferior conjunction? I hope to continue looking at Uranus.

Hopefield, Alleyn Park,
West Dulwich: 1878, March 11.

Yours truly,
GEORGE HUNT.

POLARIS.

A year, or more, ago the discovery of two new companions to the Pole-star was announced by M. De Bœe of Paris, and naturally excited much interest on the part of double star observers. These stars have been repeatedly looked for since that time, with apertures, in some instances, considerably larger than that with which it was said the discovery was made, but always without success, with the exception of one or two observers who claimed to see them in the places assigned to them by De Bœe. At the first it seemed improbable that these attendants to a star so frequently observed with large telescopes in the hands of experienced observers should have remained unseen for so long a period, and then be found with an aperture of only 6-inches. As Mr. Sadler has stated, the discovery has not been confirmed by any of the well known double star observers, and it might well be classed, without further

attention, with the mythical stars near Vega, Sirius, the Trapezium of Orion, and other familiar systems. After having failed with my 6-inch refractor, I had but little hope of finding anything with any instrument, however much larger. The 18½-inch refractor of the Dearborn Observatory was directed to Polaris last summer on at least two occasions, without showing the slightest trace of any star nearer than the well known companion of Struve; but as the nights were not particularly good, I determined to make one more attempt under better conditions. Unfavorable weather and a press of other work prevented my doing this until a few days ago. On this occasion the night was remarkably fine, and the air very steady as well as transparent. The highest power, 900, was used in measuring all the very close pairs observed during the evening, giving beautifully sharp and clear images, and widely separating pairs down to $0\cdot3$ in distance. Polaris was tried with a variety of eyepieces, but with the same result as before. It is absolutely certain that the supposed new companions have no existence whatever. There is no reasonable chance for any mistake on this point. The great equatorial is now in fine adjustment, and in its performance fully sustains the reputation of the makers, Alvan Clark & Sons. There is no star known to the catalogues too faint, and no pair too close for measurement, if measureable anywhere. In the course of the evening a number of different pairs of both classes were measured, and among close other pairs, $O\ \Sigma\ 121$ —the distance of which has never exceeded $0\cdot4$, and so difficult that even Dembowski only noted a doubtful elongation—this was beautifully separated, although the distance comes out only $0\cdot32$; 15 Lyncis, close pair, and a hitherto unnoticed small star $23''$ distant; $O\ \Sigma\ 250$, distance less than $0\cdot5$, and other doubles of a similar character. For faint objects, a very minute $4'$ attendant to a faint companion to τ Persei, $50'$ distant; the companion to μ Persei ($= O\ \Sigma\ 73$) never measured before, so far as I am aware, by any one, except O. Struve; 11. 522, which Herschel called 18 magnitude, and several others of the same class, all of which were readily seen with sufficient illumination of the wires for satisfactory measures. I mention these observations to show how impossible it would be to overlook any small stars of the kind described in the examination of the Pole-star. I would suggest a trial of some of these test objects with the small apertures alluded to in the Polaris correspondence, should there still be in the minds of the writers any idea that the stars supposed to be seen were not purely imaginary. One of the new pairs found on this evening may be mentioned, as it is very readily found, and although easily seen with the large glass, for a smaller instrument is a good test both for definition and illumination. It is a ninth magnitude star in a low power field with the well known binary, γ Virginis, $37s$. exactly preceding. The distance is a little more than one second, with a considerable inequality in the brightness of the components.

S. W. BURNHAM.

Chicago: March 3, 1878.

*LUNAR OBJECTS SUITABLE FOR OBSERVATION IN
APRIL, 1878.*

By W. R. BIRT, F.R.A.S., F.M.S.

OBJECTS SURROUNDING PLATO.—Should the evening of the 10th be clear from 8h. to 12h. an opportunity will be afforded for observing the

gradual illumination of the objects immediately west of Plato, as well as those specified in the list for March, see *ante* p. 85. Notices of such of these objects as may be detected on that evening or any future evening, as on the 10th May, when the terminator will have advanced further relatively to them, the moon's age on that evening being between 8 and 9 days, will be very acceptable, such notices may be sent direct to Buckhurst Hill. The symbols employed below mostly refer to the key plan of Plato in the volume of Reports of the British Association for the Advancement of Science, 1861, p. 183.

Moon's age, 7.79. 1861, July 15, 9h. 25m. A rugged portion of the Alps comes close up to the margin of Plato on its west flank, the eastern part of it forms the rock exactly opposite to ζ viz. δ .

A double chain of mountains, enclosing a narrow valley ζ , well seen. These mountain chains form the western border of a depression north of Plato, in which two or three clefts have been observed.

A high rock [apparently] just north of δ casts a shadow across the rim, giving the appearance of a depression in that part of it. Note.—Is this rock the δ of B. and M., or is it a rock further to the westward? The rock ζ shining very brilliantly.

Moon's age, 8.01. 1863, January 27, 5.0. Terminator through the west portion of the floor of Plato, about 0.166 from the western extremity cutting through the S.E. part of the ravine Z, which is very conspicuous; λ appears as a very brilliant spot, as also ν .

The floor of the ancient crater, Schröter's Newton, is depressed below the level of the Mare Imbrium, it is in shadow; an apparently soft ridge forming the S.W. portion of the ancient ring very perceptible; it is a gentle curve from the west extremity of the ring of Newton to Pico; the terminator has not yet reached Pico, so that it is seen at present not to join Pico. The mountain γ , at the northern extremity of the ravine Z, very distinct, it is seen as well as λ and ν as a brilliant point; a line drawn through λ and γ passes between ν and a mountain on the eastern of the two chains of mountains N.W. of Plato. See above moon's age, 7.79.

The continuation of the west rim of Plato, apparently as far as the valley l (see above), perceptible, slightly broken into close heads. I do not recognise any brighter points among them with which to identify δ and ϵ .

Moon's age, 8.03. 1863, January 27, 3.30. The west portion of the floor of Schröter's Newton becoming illuminated, Schröter's ϕ is distinctly visible as a *not over shallow crater* N.W. of ν , the west half of the interior in deep shadow, the eastern half very brilliantly illuminated. A group of mountains in the Alps, as seen in De La Rue's photogram, Feb. 22, 1858, very distinct, the west slope brilliantly illuminated.

It will be remarked on comparing these observations, moon's age, 8.01, 8.03, with those recorded under moon's age, 7.57, that similar ages of the moon do not give similar positions of the terminator, the interval from 1861, July 15d. 9h. 25m. to 1863, January 27d. 5h. 30m. is 560d. 20h. 5m., which falls short of 19 lunations by 5h. 52m., the difference of the moon's age, as given in the *Nautical Almanac* for the same interval, is 0.24, or nearly a quarter of a day. On turning to the last list, p. 85, it will be seen that at moon's age a little later than 7.57 the terminator crossed Plato; nearly 561 days later; moon's age, 8.01, it had only just passed the western rim.

Buckhurst Hill: March 14, 1878.

Erratum in last list.—Page 86, line 11, for *heads* read *beads*.

ASTRONOMICAL OCCURRENCES FOR APRIL, 1878.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage
		h. m.			h. m. s.	h. m.
Mon	1	2	Sidereal Time at Mean Noon, oh. 38m. 50 ^s .73s. Conjunction of Moon and Saturn, 5° 27' S.			β Leonis 11 2'2
Tues	2	9 14	● New Moon	3rd Tr. E.	15 58	10 58'3
Wed	3	19	Conjunction of Moon and Mercury 3° 57' S. Sun's Meridian Passage 3m. 19 ^s .95s. after Mean Noon	4th Ec. D.	16 24 14	10 54'3
Thur	4			2nd Sh. I.	15 41	10 50'4
Fri	5					10 46'5
Sat	6			2nd Oc. R. 1st Sh. I.	16 11 17 15	10 42'5
Sun	7	5	Conjunction of Moon and Mars 3° 32' S.			10 38'6
Mon	8			1st Tr. E.	15 20	10 34'7
Tues	9	9 51 10 50 2 54	Occultation of 37 Geminorum (6) Reappearance of ditto Moon's First Quarter	3rd Tr. I.	16 36	10 30'7
Wed	10	9 45 10 33	Occultation of 84 Geminorum (6½) Reappearance of ditto			10 26'8
Thur	11					10 22'9
Fri	12					10 18'9
Sat	13	6 40 7 48	Occultation of 48 Leonis (6) Reappearance of ditto			10 15'1
Sun	14			1st Ec. D.	16 19 51	10 11'1
Mon	15		Saturn's Ring: Major axis=35 ^s .87 Minor axis= 1 ^s .57 Illuminated portion of disc of Venus=0 ^s .395 Illuminated portion of disc of Mars=0 ^s .937	1st Tr. I. 1st Sh. E.	14 55 15 50	10 7'1

Astronomical Occurrences for April.

111

DATE.		Principal Occurrences.	Jupiter's Satellites.	Meridian Passage.
		h. m.		h. m. s.
Tues	16	17 57 15 38	O Full Moon Near approach of 75 Virginis (6)	3rd Sh. I. 15 26 10 3'6
Wed	17		Sidereal Time at Mean Noon 1h. 41m. 55'59s.	9 59'3
Thur	18		Sun's Meridian Passage on. 41'73s. before Mean Noon	9 55'3
Fri	19			9 51'4
Sat	20		1st Ec. R. 14 18 15 2nd Ec. D. 15 55 22	9 47'5
Sun	21			9 43'5
Mon	22		1st Sh. I. 15 50 2nd Tr. E. 15 45 1st Tr. I. 16 49	9 39'6
Tues	23	20 33 23	C Moon's Last Quarter Conjunction of Moon and Jupiter 0° 54' N.	1st Oc. R. 16 19 9 35'7
Wed	24			9 31'7
Thur	25	17	Jupiter at quadrature with Sun	9 27'8
Fri	26	16 26	Near approach of 67 Aquarii	9 23'9
Sat	27		3rd Oc. D. 14 40	9 19'9
Sun	28	0 15 19	Conjunction of Moon and Venus 3° 19' S. Conjunction of Moon and Saturn 5° 52' S. Conjunction of Neptune and Sun	9 16'1
Mon	29		2nd Tr. I. 15 25 2nd Sh. E. 15 43	9 12'1
Tues	30		1st Ec. D. 14 35 51	9 8'1
MAY				
Wed	1		1st Sh. E. 14 11 1st Tr. E. 15 31	9 4'2

THE PLANETS FOR APRIL.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	1 25 36	N. 9 34½	5".6	0 46.6
	9th	2 17 52	N. 15 55	6".6	1 7.3
	17th	2 54 18	N. 19 38	8".0	1 12.2
	25th	3 8 19	N. 20 18	10".0	0 54.7
Venus ...	1st	22 7 50	S. 8 0	36".0	21 25.5
	9th	22 29 59	S. 7 14½	32".0	21 16.1
	17th	22 55 28	S. 5 52	28".8	21 10.1
	25th	23 23 14	S. 3 58	25".8	21 6.3
Jupiter ...	1st	20 21 22	S. 19 47	34".2	19 39.3
	9th	20 25 56	S. 19 33	34".8	19 12.4
	17th	20 29 53	S. 19 20½	35".8	18 44.9
	25th	20 33 10	S. 19 10	36".8	18 16.7
Uranus ...	3rd	9 52 54	N. 13 40½	4".2	9 4.7
	19th	9 51 45	N. 13 47	4".2	8 0.6

Mercury is very favourably situated for observation. He sets about an hour after the sun at the beginning of the month, the interval increasing to two hours in the middle of the month, after which the interval decreases to an hour on the 30th.

Venus rises about an hour and a half before the sun on the 1st, the interval slightly decreasing.

Jupiter may be seen two hours before sunrise on the 1st, the interval increasing.

JUPITER.

1878 ob. Gr.	Angle of position 2½'s axis.	Latitude of Earth Sun above 2½'s equator.		Eqnat. diameter.	Defect of illumination.
April 20	344°07	—0°76	—1°10	38.11	0.37
25	343°89	0°73	1°08	38.71	.37
30	343°62	—0°71	—1°06	39.33	.38

he assumed First Meridian passes the middle of the illuminated disk at the following times :

1878. G. M. T.

	h. m.		h. m.		h. m.
April 20	17 16.1	April 24	0 39.7	April 27	8 3.3
21	3 11.6		10 35.2		17 58.8
	13 7.0		20 30.6	28	3 54.3
	23 2.4	25	6 26.1		13 49.7
22	8 57.9		16 21.5		23 45.2
	18 53.3	26	2 17.0	29	9 40.6
23	4 48.8		12 12.4		19 36.1
	14 44.2	22	7.9	30	5 31.5
					15 27.0

Assumed daily rate of rotation 870°.60.

A. M.

SATELLITES OF URANUS.

Major and minor semi-axes a and b of the apparent ellipses described by the satellites, and position-angles p_0 of the major axes:

	Ariel.		Umbriel.		Titania.		Oberon.		
1878.	a	b	a	b	a	b	a	b	p_0
April 5	14° 97	5° 09	20° 85	7° 10	34° 20	11° 64	45° 73	15° 56	0° 14
15	14° 85	5° 10	20° 69	7° 11	33° 93	11° 66	45° 37	15° 59	11° 09
25	14° 73	5° 09	20° 52	7° 08	33° 65	11° 62	45° 00	15° 54	11° 05
May 5	14° 59	5° 05	20° 33	7° 03	33° 35	11° 53	44° 60	15° 42	11° 05

The satellites will be at their greatest elongations N. (in position angle $11^{\circ} 1$) and S. ($191^{\circ} 1$), and at their superior ($101^{\circ} 1$) and inferior ($281^{\circ} 1$) conjunctions with the planet, about the following hours, G.M.T.:

Ariel.		Umbriel.		Titania.		Oberon.	
1878.	b.		h.		h.		h.
April 1	2 S.	April 1	8 N.	April 2	15 sup.	April 1	15 sup.
2	8 N.	3	10 S.	4	20 N.	5	0 N.
3	14 S.	5	11 N.	7	0 inf.	8	9 inf.
4	20 N.	7	13 S.	9	4 S.	11	18 S.
6	3 S.	9	15 N.	11	8 sup.	15	2 sup.
7	9 N.	11	17 S.	13	12 N.	18	11 N.
8	15 S.	13	18 N.	15	17 inf.	21	20 inf.
9	21 N.	15	20 S.	17	21 S.	25	5 S.
11	4 S.	17	22 N.	20	1 sup.	28	14 sup.
12	10 N.	20	0 S.	22	5 N.	May 1	22 N.
13	16 S.	22	1 N.	24	10 inf.	5	7 inf.
14	22 N.	24	3 S.	26	14 S.	8	16 S.
16	5 S.	26	5 N.	28	18 sup.	12	1 sup.
17	11 N.	28	7 S.	30	22 N.		Ariel.
18	17 S.	30	8 N.	May 3	3 inf.	April 28	19 S.
19	23 N.	May 2	10 S.	5	7 S.	30	1 N.
21	6 S.	4	12 N.	7	11 sup.	May 1	8 S.
22	12 N.	6	13 S.	9	15 N.	2	14 N.
23	18 S.	8	15 N.	11	20 inf.	3	20 S.
25	0 N.	10	17 S.	14	0 S.	5	2 N.
26	7 S.						
27	13 N.						
							A. M.

A. M.

Books received.—Astronomical Observations made at the Royal Observatory, Edinburgh. Vol. XIV. for 1870 to 1877. Edinburgh: Neill & Co. 1877.—Annals of the Astronomical Observatory of Harvard College. Vol. X. Cambridge: John Wilson & Son. 1877.—Results of Meteorological and Magnetical Observations at Stonyhurst College Observatory. 1877. Manresa Press. Roehampton, 1878.

Errata in March No.—Page 79, seventh line from bottom, for *URSI*, read *URSAE*. Page 75, fourth line from bottom, for *gauge* read *gauze*.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.		
To Dec., 1877.	To Sept., 1878.	Warner, T. Whitbread, S. C.
Browning, J. Hollis, H. W. Horne & Thornthwaite, Messrs. Metford, W. E. Pratt, H. Yeates, Messrs. & Son Yeates, Miss	Rivar, Miss	To March, 1879. Riddiford, G. F.
To June, 1878.	To Dec., 1878.	To April, 1879. Gore, J. E.
Banks, W. Z. Metcalfe, Rev. W. R.	Almond, G. S. Davies, R. P. Gerber, A. M. Leonard, Prof. N. R. Loddiges, C. Sergeant, J. P. Taplin, W. G.	To Dec., 1879. Hutchings, Rev. R. S. Rylands, T. O.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will at once inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for the Editor are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The *Astronomical Register* is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings per Quarter, payable in advance**, by postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, Angel Court, Throgmorton Street, E.C., not later than the 15th of the Month.

TRANSIT INSTRUMENT WANTED.—Any person having an efficient Transit Instrument (Second hand) to dispose of, will oblige by forwarding particulars to Mr. G. F. RIDDIFORD, Barnwood Lodge, Gloucester.

EQUATORIAL TELESCOPE FOR SALE, adjustable for any latitude, in first-rate mounting, circles divided in silver, 3½ o.g., 6 Huyghenian eyepieces, 1 transit ditto, solar and star diagonal prism, finder, strong oak tripod, for use either as a portable instrument or a fixed one.

Also, **THE OBSERVATORY**, with revolving roof and observing chair.—For particulars, apply to Mrs. SALT, 3, Denmark Terrace, Brighton.

FOR SALE.—A First-class **ACHROMATIC TELESCOPE**, with o.g. by Wray of 3½ inch clear aperture, mounted on strong pillar and claw stand, to which is attached an expensive semi-circular brass band, so that Telescope can be clamped or partly clamped in any position. Rack motion to focus, also dew-cap, finder, and 5 eyepieces, viz., 1 terrestrial, power 52 times, and 4 astronomical, powers 35, 83, 100 and 204 times. Nett Cash £19 10s.—Apply to F.R.A.S., care of W. L., 9, King's Arms Yard, London, E.C.

The Astronomical Register.

No. 185.

MAY.

1878.

ROYAL ASTRONOMICAL SOCIETY.

Session 1878—79.

The Second Meeting of the new Session, April 12th, 1878.

Lord Lindsay, M.P., &c., *President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

M. Camille Flammarion, Avenue de l'Observatoire, 36,
Paris, and

Alfred J. Shepherd, Esq., B.A., Queen's College, Oxford,
were balloted for and duly elected Fellows of the Society.

Mr. Glaisher announced that 66 presents had been received since the last meeting, and the thanks of the Society were voted to the respective donors.

Captain Abney was called upon to read a paper *On photography at the least refrangible end of the solar spectrum*. He said that nearly two years ago he had the honour of reading a preliminary note on photographing the least refrangible end of the spectrum, and the time had now come when he thought he ought to redeem the promise which was implied in the title to the preliminary note, and enter further upon the subject. Last year he was unable to pursue the subject, but during the winter and the present spring he had made fair progress in his researches. In his preliminary note he had stated that he had succeeded in obtaining photographic pictures of parts of the spectrum as far below A as A is below D. Some of these photographs would now be thrown upon the screen by means of the oxy-hydrogen light, and the Society would have an opportunity of judging for themselves whether he had over-stated the results which he had obtained. The photographic preparations which he had made

VOL. IX.

use of were sensitive to the red and ultra red rays, and they were also sensitive to the blue and violet rays, but the yellow and orange left no trace upon the plate.

The photographs were then thrown upon the screen, some of them had been taken by means of prisms, and some with a diffraction grating. In the latter the lines were beautifully sharp and the groups of lines below B were so exquisitely defined that Prof. Piazzi Smyth, to whom they had been submitted, said that they reminded him of the views of the red region of the spectrum which he had obtained when in Spain. He finally threw on the screen a photograph in which the ordinary photographic action of the red rays had been reversed, in which the absorption lines appeared opaque on a transparent ground. He explained that this was due to exposure to the acceleration of oxidation by the red rays, the plate being exposed in an oxidizing solution.

Lord Lindsay: We have had a most interesting paper from Captain Abney. I see Mr. De La Rue present, who I know has paid some attention to the subject, perhaps he would favour us with some remarks.

Mr. De La Rue: I did not intend to make any remarks, but as you have called upon me what I shall say will only extend to a question or two. Captain Abney has done wonderfully good service in the art of photography, by extending the photographic record to the ultra-red region, the ultra-red being considered amongst the least active. It will be recollected that M. E. Becquerel, some years ago, produced a spectrum in colours, but unfortunately one that would not bear the light, in which he got all the colours of the spectrum. The power which Captain Abney possesses over the chemicals which he employs, leads me to ask him whether he has made any progress in the process of producing coloured photographs. And I am going to ask another question. Latterly, a well-known man, Dr. Draper the younger, in America (the son also of a distinguished physicist, who helped to develop the daguerreotype process), has been working to obtain a spectrum of oxygen in the sun, and I would like to know for my own information, and that of the Society, whether Captain Abney can say anything with respect to this remarkable work of Dr. Draper?

Captain Abney: Mr. De La Rue has asked me about coloured photographs. In my paper read before the Royal Society, I mentioned that I thought there was a possible explanation. That paper was sent in on the 26th March. I may say that since that time I have every reason for believing that these coloured photographs are simply different states of oxidation of the photographic image, and I believe I can produce them at pleasure, but that is a matter I do not care about entering into at present. With regard to the

question about oxygen in the sun, far be it from me to pass any criticism on Dr. Draper, except upon one point, and that not on the sun but on the elder Draper, which is not on the question of oxygen in the sun. Some time last year Professor Draper kindly noticed my note as regards the red end of the spectrum, in which he said it would be very much better if I would leave off trying to take what I call proper negatives, and stick to his plan of getting reversed negatives, and suggested that some people should pay attention to the question, why the reversing action takes place. I have been for a long time considering the subject, and working at it, and the result is that I think I have satisfactorily answered Professor Draper, and that something is now known of the reason of the reverse action of the red rays of the spectrum.

Lord Lindsay: I should like to ask Captain Abney how he manages to focus for the invisible rays at the red end. I observed that on one of the negatives shown the red end and violet end both seemed to be in focus together.

Captain Abney: I think you are wrong. They are not in absolute focus. It was only a tentative experiment as to the sensibility of the blue and the red of the spectrum. I should like to call attention, while on this subject, to Professor Piazzi Smyth's map, which I dare say you know, and I hope it will be compared with the photographs. I think you will see there are more lines than Professor Piazzi Smyth has drawn, and in the ultra-red there are lines which I suppose nobody has ever seen at all, and in fact I did not see them myself until a short time ago, and I was surprised to see the beautiful grouping. When you come to a wave length of 12,000 the groupings are very remarkable, so much so that I have hesitated to bring them forward to-night at all.

Lord Lindsay: One other question I would ask: I should like to know if Captain Abney has made any measurements with the thermopile regard to the locality of these lines, and whether he has found any aggregation of them at the red end?

Captain Abney: I am now undergoing a process of pumping on immature experiments. I do not mind being pumped to a certain extent, but I may say that these experiments are going on with the others, although they are not in a fit state to be brought on to-night, so I did not like to mention them. But the focussing of the ultra-red is a matter of the very greatest difficulty and trial. It was only last night I found that somebody had moved my condenser in my laboratory, and when I wanted to get a photograph I found that it was all wrong, and I could not get one that was anywhere in focus. Focussing is merely a matter of trial and experiment.

Mr. Ranyard: I would like to ask whether all the photographs you have shown—especially one with the remarkably sharp lines about the region of B—were actual photographs of the spectrum without any touching or alteration.

Captain Abney: I am perfectly willing to answer that question. It was a genuine photograph of the spectrum. It was suggested to me that some people might consider it a photograph of a drawing, but I think I need hardly mention here that it is a true photograph of the spectrum.

Mr. Ranyard: The photographs were, I understand, taken with a diffraction grating. I should like to ask in what order of spectrum they were taken, and how have you isolated the particular spectrum shown on the photograph.

Captain Abney: I tried to explain that in my paper. I separated them by the old method, by using a prism at right angles.

Mr. Ranyard: But how did you manage to cut out entirely the other orders of spectra so that they did not even appear on the plate?

Captain Abney: Some of them are on the plates, but I did not show them on the screen.

Lord Lindsay: I think the Society will give a most cordial vote of thanks to Captain Abney for his most interesting communication. I will only add that when communications of such interest come before the Society, the Fellow making the communication must not mind a little cross-examination. "Pumping," no doubt, is not always pleasant, but it is very pleasant to those who get the information. I will ask you to pass a vote of thanks to Captain Abney. (Applause.)

Mr. De La Rue: I will only make one remark—Captain Abney's well is not dry. (Hear, hear.)

Mr. Ranyard called upon the Astronomer-Royal to read a communication which he had received from M. Fizeau, President of the Academy of Sciences, in reference to a committee which had been formed for the erection of a statue to M. Le Verrier.

The Astronomer-Royal stated that the communication had been sent to him to bring before his scientific friends in England, and with that object he had communicated it to the Society.

Lord Lindsay then read the communication, which was in the form of a circular from the president of the committee for the erection of the statue, stating that a committee had been formed to honour the memory of M. Le Verrier, and to get together voluntary subscriptions to raise a statue or other monument to his memory. The President added that it would be as well that this communication should be published in the *Monthly Notices*,

and any Fellow of the Society who might desire to contribute would be made aware of the channel through which he could do so.

The Astronomer-Royal: In reference to this matter I think it desirable the Society should consider what has been done in other cases of a similar character. Two or three instances occur to my mind. One was the erection of a monument to Sir Isaac Newton at Grantham. I am not aware that any step was taken towards soliciting contributions from foreign men of science on that occasion. I speak, of course, under correction, and only of what has come under my own knowledge. What was done then was, I believe, done entirely by Englishmen. There is another instance, namely, the memorial to Gauss, and although circulars soliciting subscriptions were addressed to many persons in England, I do not think much, if anything, was done in England in aid of the memorial. It seems to me that the best feeling to be entertained in matters of this kind is a feeling of local pride with regard to the men who have distinguished themselves in science in any country by the scientific persons of that country. I speak of this naturally as merely my own feeling. There is one thing of which I am quite sure, that any person who knows what has passed in the last twenty or thirty years in the intercourse between the two nations, can have nothing but the highest opinion of that intellectual giant, Le Verrier. (Applause.) He was a great man, and I am speaking entirely in an abstract manner with regard to the exclusive claims each country has to the celebration of the men of science of its own nationality.

Lord Lindsay: A suggestion was brought forward at the Council that the Society, *qua* Society, should subscribe to this memorial, but it seemed to be thought that the Council could not according to its bye-laws devote any of its funds to such an object.

Mr. Ranyard read a short paper by Mr. Ellery, *On observations of the polar and equatorial diameters of Mars*, as observed near its opposition in 1877. The observations were made with an equatorial instrument eight inches in diameter.

Mr. Christie said he had received a letter from Mr. Ellery, in which he mentioned that he had made a number of drawings of Mars, which he would be happy to send to the Society if they were likely to be of any use. (Hear, hear) But at the same time, Mr. Ellery added, that the definition was very bad indeed.

Mr. B. G. Jenkins then read a paper *On the luminous spot seen on Mercury in transit*. After referring to the records of observations of transits of Mercury made during the last 170 years, he came to the conclusion that—

1. In the May transits when Mercury is near aphelion the luminous spot is in advance of the centre of the planet, whereas in the November transits, when the planet is near perihelion, it appears that the luminous spot follows the centre.

2. The luminous spot has never been seen at the centre, but always a little south of it, and it cannot be due to diffraction.

3. In some of the transits two spots have been seen close together, where shortly before only one was observed.

4. In the May transits the rings round the planet are dark and nebulous, while in the November transits they are bright.

In the two transits most accurately observed, namely, those of May, 1832 and November, 1868, the contrast was found to be very great and typical. In the 1832 transit there was a diffused spot preceding the centre with dark rings round the planet, while in the 1868 transit there was a sharply defined spot following the centre with bright rings surrounding the planet. Mr. Jenkins, therefore, concluded that on the 6th of May next, when Mercury will be near to aphelion, the luminous spot will not be a point of light with no sensible diameter, but a diffused spot rather to the south of the centre, and the planet will be surrounded by dark nebulous rings and not by bright ones.

The Astronomer-Royal said—I am somewhat surprised that no notice of the observations made at the Royal Observatory has been taken by Mr. Jenkins. The observations which have been made at Greenwich are given in the books which are published annually, and observations of transits of Mercury have been made there on more than one occasion.

Mr. Chambers asked whether Mr. Jenkins' paper, if published in the *Monthly Notices*, could not be issued before the 5th of May next.

Professor Cayley said it was almost impossible to bring the monthly notices out short of the month. The delay in the last issue was partly accounted for by its being a heavy Number. It was true he had had five weeks, but next month would be a month of four weeks, and there would be the interruption of Easter, though he would endeavour to bring it out as early as possible; and he thought it could hardly be done before the time of the next meeting on the 10th.

Mr. Chambers suggested that if the *Monthly Notices* could not be got out before the 6th of May the paper on the transit of Mercury should be published separately and distributed, at any rate to those Fellows who had observatories or instruments.

Professor Cayley said that course had frequently been followed in other cases.

Lord Lindsay said he apprehended it would be quite within

the power of the meeting to order any paper of peculiar importance to be printed and circulated amongst the Fellows. He had intended to propose to the Council that some of the gentlemen who had observed the transit of Venus should be asked to act together in drawing up recommendations to astronomers in this country as to the best method of procedure in making observations of the coming transit of Mercury. He regretted that such recommendations had not been drawn up. He believed, however, that the American Government had issued a circular on the subject which he supposed would be generally seen.

Mr. Ranyard said that the circular alluded to referred principally to observations of the time of contact, but it was also of importance, and possibly of much greater importance, that the physical phenomenon, such as Mr. Jenkins had alluded to, should be very carefully studied. Any suggestion with regard to observations to be made of the spot or spots of light near the centre of the planet would be of considerable interest, as there seems to be a great deal of mystery about them, and it is by the study of such mysterious and apparently inexplicable phenomena that new facts are frequently brought to light.

Mr. Knobel said with regard to the circular from Washington, it was mainly directed to suggestions for inexperienced observers, detailing the methods that should be adopted for securing observations of the time of ingress. The pamphlet was accompanied by a diagram, which it was suggested should be placed at a distance of half a mile and examined, when it would correspond to the apparent size of Mercury on the disc of the sun.

The Astronomer-Royal said that he had at the Royal Observatory at the present time a large number of instruments which had been used in observing the transit of Venus. He was not at liberty to allow any of them to go out of the precincts of the observatory, but if a number of gentlemen were disposed to watch that portion of the transit which would be observable on the 6th of May next, and would be disposed to view it within the observatory grounds, he could afford them the accommodation of about ten telescopes. He believed five of the telescopes were of six inches aperture and the others about four. If gentlemen were disposed to come he would locate them in the best positions he could if they would give him a fortnight's notice, but he could only give them out-door accommodation, say for about ten telescopes with a chronometer to each.

The President said if the Society would place confidence in him he would endeavour to get out a circular such as appeared to be wanted. On the part of the Society he felt sure they would accord their warmest thanks to the Astronomer-Royal for his

generous offer. If he had not been going to Scotland he should have been among the first to have asked for the loan of one of the telescopes.

The Astronomer-Royal said he wished it was in his power to authorize the distribution of these telescopes, but he was not allowed to part with them. He mentioned that in 1842 he was successful in observing the eclipse near Turin, while some of his friends within a very short distance saw nothing owing to the clouds.

Mr. Green showed some drawings of the planet Mars which he had received from Professor Schiaparelli, of Milan. As the letters accompanying the drawings brought up a question of great interest, Mr. Green thought he ought to bring the matter before the Society. If the drawings were correct, they indicated that great physical changes had taken place on the surface of the planet since the last opposition. They showed several dark channels through the lightest portions of the planet, which were not visible till some considerable time after the opposition. These phenomena were seen in October and the following months, and it was not till then that the equatorial zone of the planet began to be broken up into channels, which intersected one another. Although these details were very minute, it appeared that M. Schiaparelli had continued to observe them up to the 17th March, 1878. The drawings sent appeared to be combination drawings, and not pictures of the planet as it appeared at any one time. Mr. Green hoped he should be excused if he exercised a little artistic criticism on the drawings. He thought the hard and sharp lines must be an error, and were the result of some process which Prof. Schiaparelli had adopted in making the drawings. Again, he thought there must have been a tendency, either in the object-glass, or the eyepiece, or in the eye of the professor, to elongate and develope dark points seen against a light background.

Lord Lindsay said he understood these drawings were the outcome of a series of sketches, and not the actual pictures of what Schiaparelli had seen on looking through his telescope, but merely the massing of various details he had seen at different times into a map.

Captain Noble referred to the drawings which he had some time ago laid on the table, on the occasion when Mr. Green also exhibited his exquisite ones. He was sorry to say somebody must have taken his drawings away, for they had never been seen since. Whoever it was he thought they exhibited very bad taste in taking his drawings and leaving those of Mr. Green (laughter.) However, the point he wanted to mention was that he had watched Mars sedulously, and he had never seen the reticulations shown on Prof. Schiaparelli's drawings.

Mr. Green reminded Captain Noble that the Professor stated that the markings were not visible at the last opposition, but had only become visible during the months of January, February, and March of this year.

Captain Noble intimated a doubt whether such marks could be seen when the diameter of Mars subtended an angle of only 5 or 6 seconds.

Lord Lindsay said that Professor Schiaparelli was not likely to be led away by imagination. There might be something peculiar in his telescope, or in his eyes, but he was not likely to publish observations or drawings without being fully persuaded that the appearances actually existed.

The President then drew attention to a large oil painting which was exhibited in the room, representing sunset upon a lunar crater. The picture was criticised from an astronomical point of view by several gentlemen.

The following papers were also laid upon the table :

R. P. Greg : *On the duration of meteor showers.*

E. J. Stone : *A comparison of the observations of contact of Venus with the sun's limb in the transit of 1877, Dec. 8, made at the Royal Observatory, Cape of Good Hope, with the corresponding observations made at the stations in the Parliamentary Report, and a discussion of the results.*

J. R. Eastman : *Observations of the companion of a Canis Majoris with the transit circle of the U. S. Naval Observatory.*

Rev. S. J. Johnson : *Early transits of Mercury.*

Lord Lindsay : *Note on the Table p. 70, Vol. II. of Dun Echt Observatory Publications.*

The meeting adjourned at a quarter to ten.

REVIEWS.

Dun Echt Observatory Publications. Vol. II. Mauritius Expedition, 1874.

Division I. *Determination of the Solar Parallax by observations of the minor planet Juno (3) at opposition; together with a description of the Heliometer used in the observations.* Dun Echt, Aberdeen, 1877 (4to. pp. 212).

In the epistle dedicatory Lord Lindsay says:—"Had it not been for the interest my father has always taken in my pursuits, I should not have been able to undertake a work of such magnitude, nor would these pages ever have appeared." We are told in the preface that the preparations for observing the transit of Venus, commenced some time before, were terminated in the spring of 1874. The chronometric and telegraphic determinations of the longitude of Mauritius, in the former of which fifty-three chronometers were employed, are to be reported with other matters in another volume, and the details of the observations of the transit of Venus proper, comprising those made with the heliometer and the 6-inch equatorial, and the account of the photographic arrangements and discussion of the negative obtained, in the fourth volume.

In the introduction the method employed for determining the solar parallax is described in its simplest form, as consisting in "measuring the position angle and distance of a minor planet relative to a star in the evening and similarly in the morning." The difference of results corrected for motion of the planet gives the data for computing the horizontal parallax. The total parallactic displacement is very considerably less than in the case of an opposition of Mars, or a transit of Venus, but we believed that the great accuracy attainable in the heliometric measurement of the distance of two minute points of light would more than compensate for this defect. We think these anticipations have been justified by the result, and as the best method for a rigid determination of the solar parallax is one of the most important, as well as one of the most generally interesting, questions which are presented to the astronomer, we have thought it best to submit the following investigation as first fruits of the expedition. It is distinctly to be understood that the result arrived at is not presented as an example of the accuracy of which the method is capable, but is to be regarded merely as a first imperfect experiment, made under somewhat unfavourable and trying circumstances, from which conclusions can be drawn as to the capabilities of the method for the future.

(Signed)

LINDSAY.

DAVID GILL.

Chapter I. is about the *heliometer*, which was constructed by Messrs. Repsold, of Hamburg; the equatorial stand and clockwork were made by Messrs. T. Cooke & Sons, of York; the design of the instrument had been carefully considered by Drs. Auwers, Winnecke, and Struve. This superb instrument, whose object-glass, by Merz, is of 4.2-inch aperture, focal length about 64 inches, and of definition in the highest degree admirable, is minutely described with illustrations. Ch. 2 is an elaborate account of the *investigation of the errors of division of the heliometer scales*, and takes up 40 pp., yet it is but the *résumé* of details that fill a folio volume. The conclusion is that for the heliometer in question it is possible to find each division error with an accuracy from $\pm 0''.02$, to $\pm 0''.03$, and the maximum probable error of the measured apparent distance traversed by Juno between the evening and morning observations is $\pm 0''.040$, or for a complete double observation $\pm 0''.014$. Ch. 3 is a description of the *errors of the screw*, with results as follows:—probable error of the determination of the length of a screw revolution $\pm .00031$. Extreme probable error, being the accumulation of the above over two revolutions, $\pm 0''.011$. Correction for periodic error with a probable error of $\pm 0''.005$. The sum of these is only $\pm 0''.016$, and as each complete measure involves 16 screw-readings at 8 generally different points of the screw, it is exceedingly improbable that any systematic errors amounting to $\pm 0''.01$, due to uncorrected screw error, can arise. (p. 60.) Ch. 4 is on the method of observation and calculation of instrumental errors, with subsidiary tables. Ch. 5, is about the *observations at Mauritius*, of which the first series of value was made on November 12, and observations were secured altogether on twelve evenings and eleven mornings, between that date and November 30. The completion of the Juno series was followed by observations on two zones of stars, to determine the value of the scale, measures of stars for the zero of position circle (according to the programme of Auwers and Winnecke), &c., longitude and latitude determinations, and preparations for the chronometer expedition homewards. Ch. 6, contains, 1. *Table of Star observations*; 2. *Table of results of Star measures*; 3. *Table of observations of Juno*. Ch. 7, is on the *value of the Scale, and determination of the zero of the Position Circle*. For the angular value of the scale, or normal screw

revolution two zones of stars were observed at Mauritius, one in Perseus, the other in Hydra. "These zones have also been observed by the other heliometer observers, and the places of the component stars have been determined by meridian observations at Berlin, Leipzig and elsewhere. The whole including a few meridian observations of the Perseus zone made at Dun Echt, were communicated to Dr. Aowers, who discussed them and communicated the results. Ch. 8, contains *Tabular distances, Position angles and factors for Juno, relative to stars of comparison*; an extensive set of tables. Ch. 9, is the *Comparison of observed and tabular distances and position angles*; another extensive table. Ch. 10, is on the *Formation of the Equations of Condition*. Ch. 11, *Correction of the Equations for errors in the Tables of Juno*. For the places of Juno, three series of observations near the opposition of 1874 were used; Greenwich meridian observations (communicated by Sir G. B. Airy); Washington meridian observations (*Astronomische Nachrichten* 2,078); Cambridge, U.S. observations with equatorial and comparison stars (*Astronomische Nachrichten*, 2,051); and to correct the star-places of the last mentioned, Mr. A. Searle, assistant in Harvard College observatory, communicated observations of comparison stars for places of Juno, made with the meridian circle at Harvard College observatory. Finally, in Ch. 12, we have the *Solution of the Equations, and deduction of the parallax*. The Washington and Cambridge series of observations, and those of Greenwich slightly differing, the observations were reduced on both systems (p. 204). The assumed value of the mean solar parallax was $8''.85$; hence the values resulting are—

1st. System, $8''.77 \pm 0''.041$; 2nd. System, $8''.76 \pm 0''.042$.

"The values of these probable errors depend on too few observations to be trustworthy; nor do we ourselves attach high importance to the value of the parallax deduced. But that to which we do attach great importance is the very remarkable accuracy of the method when this value of parallax is substituted in the equations—as in the following table of the residuals which represent the errors of the observations of distance. [Then follows the table.] The resulting probable error for a complete observation is from this table on the first system, $\pm 0''.073$, and on the second system, $\pm 0''.082$, confirming, beyond all doubt, the conclusion at which we arrived . . . that it is possible, by means of the heliometer, to determine the place of a minor planet relative to two stars (or if within a distance of $1,000''$, to one star), with a probable error of less than a tenth of a second of arc. Considering that it was impossible to commence observations until a week after opposition, and that in consequence hardly one-third of the anticipated observations were secured, and these in circumstances the least favourable for parallax factor, we do not attach very much importance to the value of the parallax deduced. But we have at least now sound data upon which to found calculations as to the value of the opposition of any minor planet for the future determination of the solar parallax." In a note it is stated that "there is a discordant value on November 15, which in a larger series would probably have been counterbalanced by another, in which all the sources of error had combined in the opposite way; and thus, if we were asked what we believe to be the most probable value resulting from the determination, we should reject this result; the values then become—first system, $8''.82$; second system, $8''.81$. At the same time we are aware that the rejection of any observation is quite unsound."

Lord Lindsay was accompanied by Dr. Ralph Copeland, Mr. Henry Davis, and Dr. Blackley the surgeon of the expedition. He went round by the Cape in a schooner. Mr. D. Gill, with the chronometers, went out

by the Red Sea. The great kindness all met with from the inhabitants of Mauritius is gratefully recorded: amongst a few names mentioned, is that of Mons. de Chazal, towards whom and his family Lord Lindsay says—"I shall ever bear the warmest feelings of regard and sympathy. He most generously placed at my disposal, during my stay, the house and buildings on his estate of Belmont, in the district of Poudre d'Or, in the north-east part of the island, about sixteen miles from Port Louis. When prostrated with fever, had I been his own son I could not have been cared for with more tenderness." The same gentleman made over to the Government the site of the observatory, that in the interests of science, the actual spot where the observations were made should be preserved, and should not share the fate of the *Base*, measured in 1753-54, by the Abbé La Caille, which, unfortunately, cannot now be identified. On this ground, says Lord Lindsay, "I claim for Mons. de Chazal the gratitude of future generations of astronomers and geodeticists."

The volume we have endeavoured to describe has excited our warm admiration. It is not too much to say that it would be worthy of any first class observatory; we might venture to affirm that it might have come from the illustrious Bessel himself. From the graceful dedication to the modest summing-up of the result of so much skilfully directed labour, all seems deserving of unqualified praise. Astronomy has been indebted to the wealth of some who pretended to little personal knowledge of it; still oftener has it owed its advances to the science of others who have contended against the *res angusta domi*. It is truly delightful when we witness the combination of rank, wealth of resources, leisure, science, and devotion to its pursuit in the same person. This volume of Dun Echt Observatory Publications will, probably, mark a sort of era in the history of the great problem of the solar parallax. The little planet discovered seventy years before by Harding has obtained an importance of which he, probably, never dreamed. It will be curious if the problem should be ultimately solved within the narrowest limits by the observation of these tiny bodies—"minute points of light" in the telescope. Even had their existence been known in the last century, and their applicability to this purpose been thought of, it would still have been necessary to wait for the mechanical and optical skill of such artists as Repsold, Merz, and the Messrs. Cooke. And that so much practical success has now been achieved with an instrument, the product of their joint workmanship, is to a great extent due to the science possessed by those who used it: who, whilst doing justice to its superlative excellence, rested not till they had discovered and reduced to measurement its every secret imperfection. We are also reminded in this work of the indispensable aid furnished by the great centres of astronomy, not only in England, but in America, and on the Continent, on which the extra-meridional observer must rely for the ultimate value of his observations. And the co-operation of Drs. Anwers and Winnecke, repeatedly referred to in this volume, has been of the greatest value. Many distinguished names are thus seen to have been associated with the Mauritius expedition; and, whilst not pretending to ascribe to each person his portion of credit, we may remark that the heliometer produced by a triumvirate of opticians has well proved its excellence in the hands of a triumvirate of observers; and we believe that no others could have better brought out its powers, better provided against its inevitable shortcomings, and more skilfully discussed the observations made with it, than Lord Lindsay, Mr. D. Gill, and Dr. Copeland. When we say that Messrs. Spottiswoode & Co. are the printers it is superfluous to add that the type, printing, and paper of this volume are excellent.

The Divine Order of the Universe as Interpreted by Emanuel Swedenborg, with Especial Relation to Modern Astronomy. By the Rev. Augustus Clissold, M.A. Longmans, 1877. Pp. 121.

An article by Mr. Proctor in the "Belgravia Magazine" for September, 1876, on "*Swedenborg's Visions of other Worlds*," gave occasion to the present work. "It would almost seem," Mr. Proctor wrote, . . . "that the system of Swedenborg gave no place to Uranus and Neptune, simply because he knew nothing about those planets. Otherwise what a noble opportunity there would have been for establishing the truth of '*Swedenborgian doctrines*' by revealing to the world the existence of planets hitherto unknown." We are inclined to think with Mr. Clissold that such a sign as the one demanded would have been worth little as evidence of Swedenborgian doctrines, and were this a fitting place we might give other reasons besides those he adduces for thinking so. He writes (p. 21): "The religious difficulty, then, still remains, and astronomy has yet to go in search of a religion. But is it a satisfactory state of things that the grandest of all sciences, resting upon universal and immutable laws, should either find nothing in Christianity to which to ally itself but religious difficulties, or else the shifting speculations of individual opinion? And he thinks that the system of theology supplied by Swedenborg precisely meets this difficulty, and proceeds to explain and apply its principles. On the plurality of worlds he quotes from Chalmers, Whewell, Brewster, and the authors of the '*Unseen Universe*,' as well as from Mr. Proctor's works, and unfolds the visions of Swedenborg, who, to meet the unbelief of his age about a future life, &c., affirmed that the interiors of his spirit had been opened by the Lord, so that he was enabled while abiding in the body to have intercourse with the angels in heaven; and not only to discourse with them, but also to see the astonishing things of their kingdom, and to describe the same, in order to check from henceforth the cavils of those who urge—'Did ever any one come from heaven and assure us that such a place exists, and tell us what is doing there?' " (p. 68). After being led by divine guidance to his several destinations in the world of spirits, he is left to collect his information from the spirits who were there, especially from their external memory; these spirits having previously belonged to their respective planets, for it was not with the inhabitants themselves of the planets, but with the spirits and angels who had come from thence that he was permitted to discourse. As to the relation of these spirits and angels to the visible or material universe, in their ordinary state they neither recollected, nor saw, nor knew anything concerning it. Having lost by death the material body they had lost the use of its bodily senses, and can see nothing that is in the natural world; for the solar light is to them as thick darkness. If then spirits and angels are in such total ignorance with regard to the state of the material universe, how is it possible that any information upon this subject could be derived from them? The answer is—it was obtained from their external memory, which in the spiritual world lies dormant, and is never awakened except only for special purposes and on special occasions (pp. 70—72). It would seem, then, that the reason why Swedenborg failed to elicit such information as some think would have served as evidence of the truth of his system was owing to the inability of that eminent man suitably to cross-question the extraordinarily constituted spirits with whom he believed it was given to him to have intercourse. And were another to arise who should claim to be similarly gifted we should still apparently, as before, have to depend solely on the weight that might be attached to his own assertions. There is no more external evidence for the visions of Swedenborg than for those of Mohammed, who believed that he received on Mount

Hira his divine commission from Gabriel, sent to him from God; and who, by some deceptive mental process, appears to have sincerely believed in his own inspiration, and whose book is regarded by his followers as inspired in every word. Nevertheless, we decline to admit the authority of the Arabian prophet, and there are also quite sufficient reasons, with most at least, for rejecting as baseless the system and the visions of the Swedish seer. We must add that, according to him, there are no such things as infinite space, infinite time, or infinite matter. Whilst agreeing with the author of this book and his master, in so far as opposition to "naturalism" goes, we find ourselves as far as ever from being persuaded that the system he advocates is adapted to meet it, or that it possesses any claim to the serious attention of astronomers.

Miscellaneous Papers connected with Physical Science. By Humphrey Lloyd, D.D., D.C.L., Provost of Trinity College, Dublin, formerly Professor of Natural Philosophy in the University. Reprinted from the "Transactions of the Royal Irish Academy," the reports from the British Association for the Advancement of Science, &c., pp. 509. Longmans. 1877.

We have perused this work with much interest. The first portion, pp. 1—169, consists of articles on physical optics, the titles of which are: 1. On the phenomena presented by light in its passage along the axes of Biaxial crystals. 2. Report on the progress and present state of physical optics. 3. On a new case of interference of the rays of light. 4. On the light reflected and transmitted by thin plates. Then follow magnetic observations, theories, tables, formulae, &c., pp. 170—311. Meteorology and climate of Ireland, and currents of the Atlantic, &c., pp. 312—413. The rest of the volume consists of lectures and addresses. The dates of the papers range from 1834 to 1865. Those acquainted with their value will be glad of their reproduction in this volume, to which an index might have been desirable. Besides many diagrams there are seven plates—magnetic chart of Ireland, earth currents, meteorology of Ireland. Dr. Lloyd's name stands high in science. His treatise on light and vision, and on the undulatory theory of light are probably known to many of our readers. The long paper, No. 2 in this collection (*Report of the British Association for the Advancement of Science for 1834*) is, we think, particularly valuable. The different steps, experiments, and reasonings which established the wave, in preference to the corpuscular, theory of light, and the great names associated with the controversy, are brought before us in a very able and lucid manner. But in fact the whole volume consists of matter of permanent value, and reflects honour on the Royal Irish Academy, which amongst other distinguished members has counted such men as Brinkley, Sir William Hamilton, and Professor MacCullagh.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

SATELLITES OF JUPITER.

Sir,—Mr. Gardiner will find the exact times of the first conjunctions of the satellites, for the beginning of each year from 1880 to 1890, in the

Appendix to *E, Nautical Almanac* for 1881, together with the arguments, &c., so that the tables in Vol. III. of Vince's *Astronomy* could also be used.

I would give the times, &c., here, but it would occupy too much space. I must leave it to someone else to give the same for Saturn.

I may also mention that Damoiseau's Tables of Jupiter's Satellites run out in 1880.

2, Campsbourne, Hornsey, N.

I am, &c.,

R. T. A. INNES.

ARTIFICIAL HORIZON.

Sir,—In reference to the short article on this subject in the *Register* for March, a Mr. Samuel Dunn, who observed the transit of 1761 at Chelsea (and who formerly observed at Crediton), states that he used an artificial horizon of sweet oil in a tea saucer as preferable to quicksilver, see *Philosophical Transactions*, Vol. LII. I have often employed a saucer of olive oil myself as an alternative for a quicksilver horizon, and with advantage, though it certainly does not seem superior to quicksilver for the purpose.

A darker substance than olive oil, as e.g., treacle, would hardly serve for sextant purposes, as one of the images would be too faint.

Upton Helions Rectory,

Yours faithfully,

Crediton: March 20.

S. J. JOHNSON.

THE PROPER MOTIONS OF THE EDINBURGH STAR CATALOGUE.

In Vol. XIV. of the *Edinburgh Astronomical Observations*, 1870—1877, Prof. Piazz Smyth has attempted to correct the proper motions of the British Association Catalogue by observations made at Edinburgh and elsewhere, from 1830 to 1870. Prof. Smyth seems to be possessed of the strange idea that the British Association Catalogue is still considered a high authority, both for position and proper motions, and does not appear to be aware of the fact that it is, and has been for some 10 years past, acknowledged (I was going to say universally) that one of the most pressing wants in the department of sidereal astronomy is a new general catalogue of the character, as regards extent, of that published by the British Association, which does not worthily represent the accuracy of modern observational astronomy; and in no respect is it more defective than in its proper motions, which are determined from too short intervals, as well as, in many cases, from not very accurate places. It is evident that Professor Smyth's proper motions labour under the former of these defects which, even if the observations were of an irreproachable character, would be detrimental to their accuracy; but when in addition we take into consideration the "swervings" of the Edinburgh transit instrument, it is impossible to regard them with any degree of confidence.

I think there can be no doubt that the best determinations of proper motions that we have as yet are those given by Mr. Stone in the 33rd Vol. of the *Memoirs* of the Royal Astronomical Society. These proper motions are derived from a comparison of the observations of Bradley, *vir incomparabilis*, as given in Bessel's *Fundamenta Astronomiæ*, with the Greenwich Seven-Year Catalogue for 1860. The epoch to which the observations of Bradley are reduced being 1755, this gives the long interval of 105 years for the determination of the proper motions, and this, combined with the great reputation of Bradley as an observer,

makes this memoir the most valuable contribution to our knowledge of this branch of astronomy that has yet appeared.

I have selected from the Edinburgh Catalogue a few of the stars, the quantities of whose proper motions Professor Smyth proposes to alter considerably from those given in the British Association Catalogue, and from this list I have picked out the stars whose proper motions have been determined by Mr. Stone, and have thus compared the values as given by the three authorities.

I give the comparison for the first three stars on the list:—

Star's name.		Proper motion according to		
		B. A. C.	Smyth.	Stone.
6 Ceti	R.A.	—'004	—'006	—'009
	N.P.D.	+ '22	+ '33	+ '27
11 Ceti	"	+ '016	+ '008	+ '008
	"	+ '02	+ '08	+ '05
λ Cassiopeiæ	"	+ '006	+ '008	+ '003
	"	—'02	+ '08	+ '02

If these are fair samples of Professor Smyth's work it appears that much reliance cannot be placed on it, and that he has done but little even towards improving the proper motions of the British Association Catalogue.

A. W. DOWNING.

3, Park Terrace, Greenwich.

JUPITER'S ATMOSPHERE.

Sir,—Beyond informing Mr. Neison that his reasoning and his conclusions are alike paradoxical, and recommending to him the study of the physical researches of Despretz, Dulong, Cagniard-Latour, Andrews, Petit, Arago, Régnault, and others, I can waste no more of my time in connection with his lucubrations on this subject. I may, however, suggest, as an interesting problem for him, the inquiry into the probable depth of a planet's vaporous envelope, if the temperature of the planet were above the critical temperatures of the elements forming the greater part of the planet's mass.

I must disclaim any "prejudices" against Mr. Neison. It is not, however, my fault if he has succeeded in removing those of a different kind, the only prejudices I ever entertained respecting him.

I find reason to believe, however, from his last letter, that he ought "not to be held responsible" for the rude things he says (mixing them with complimentary expressions, somewhat as Mr. Sampson tells Mrs. Wilfer, "with the highest respect for her," that she is "a demon"). I suppose, for instance, he hardly meant to say in his last, that "no one but Mr. Proctor would think of doubting" such and such a thing.

But I can promise Mr. Neison not to doubt anything he may say hereafter, for I will avoid reading matter of his inditing—at least if he will only refrain from writing anonymously.

Faithfully yours,

RICHARD A. PROCTOR.

53 VIRGINIS.

This star is entered in one of the double star catalogues of Sir John Herschel with a companion, the magnitude of which is not given, in the direction of $30^{\circ} \pm$, at a distance of $50'' \pm$, with a note, "position by

diagram." Smythe in the "Cycle of Celestial Objects" gives the small star 15 magnitude, angle 35° , and distance 45", but states the companion was only glimpsed, and that his results are only estimations. I have looked for this several times in the past five years with the 6-inch refractor under very favorable circumstance, but was never able to see the least snispicion of any star in Herschel's place. Mr. Sadler in a letter in the *English Mechanic*, some two or three years since, I believe stated that he had suspected the small star on one occasion. Beyond this, I know of no other observations of any kind of 53 Virginis as a double star.

I examined this star with the 18½-inch refractor on the evening of March 28th, and measured a small star as follows:—

$$P = 9^\circ.3.$$

$$D = 70^\circ.56.$$

As will be seen, this differs widely from the Herschel star both in angle and distance. There is certainly no physical relation between stars of this class, and if any change has occurred it must be explained by the proper motion of the principal star. Wide differences between Herschel's estimated distances and subsequent measurements, are by no means uncommon, and the want of agreement in the estimated angle would be easily accounted for by the diagram used. The observation of Smythe can have but little weight in deciding the question of change, as in several instances he has measured small companions in places where they did not exist, as, for example, γ Persei and δ Cancri. But in all these cases his measures accord perfectly with the erroneous observations with which he compares his own. It is very improbable that this star could have been seen by him with his instrument under any circumstances, for it is much fainter than any star I now recollect of having been certainly seen by him.

Chicago: April 8.

S. W. BURNHAM.

COMETS.

Sir,—I am unable to agree with Mr. Lawrence's theory of comets. The first difficulty I find is to account for the form of the tail. If, as Mr. L. says, a comet is a spherical lens, the rays of light should follow the law of all rays of light, passing through such a lens and converge to a point (the focus of the lens), in which case the tail would be a cone, with its broadest end at the nucleus. Perhaps Mr. L. will say that is the case, and that after crossing the rays spread out and form the tail. But here I fear we are no better off; if the rays cross we shall get the same amount of light at the exact distance from the focus that we have at the nucleus, after which the light will be less intense than in the surrounding space, from the dispersion of the light over a greater space. Mr. L. must remember this would not be like a lens inserted in a screen, and the rays of light passing into darkness. This would be a lens held up in the sunlight, and simply condensing the rays immediately in its wake. With regard to the extreme lightness, little can be said in proof or disproof. If all the comets ever seen combined do not weigh an ounce, it appears to me just as likely they do not weigh a grain. One thing is certain, they are subject to the laws of gravitation, and, if I mistake not, gravitation and weight are in this case convertible terms.

Now, with regard to the curve in the tail, if the tail is illuminated star dust in the path, the curve would be the curve of the path, and that being an extremely long ellipse, for the greater part of its journey the tail would be practically straight, whilst when rounding the sun it would fall out of the path of star dust, and disappear altogether. If the

curve in Donati's comet had represented the curve in its path, it would have turned completely round, and returned to the sun each time it had travelled eight or ten times its own length. How does Mr. L's theory account for the six tails of the comet of 1744? My theory of a comet is that it is a gaseous body in a luminous and highly electrical condition, that the lighter part trails away from the denser, and that being electrified in the same way as the sun, the tail always trails away from that body, because like electricities repel each other.

Hoping Mr. Lawrence will not consider this anything but a fair criticism on his theory,

London: March 6, 1878.

I am, obediently yours,

W. G. TAPLIN.

ω^1 CANCRI.

Sir,—The March number of the *Astronomical Register*, contains a letter from Mr. S.W. Burnham, in which he suggests that the double-star observed by Mr. Buckingham and myself as ω^1 Cancri was not that star, but a pair nearly 2° south of it, viz., Cancri 5 B (Σ 1171). I looked at this pair on the evening of the 12th instant, and found it to agree fairly with the requisite conditions, though the distance seemed a little wide, and the colours not so marked as I should have expected from the wording of the note in my observing book, and from my memory of the observation. The atmospheric circumstances, however, were not very favourable, and bright moonlight, combined with cloud and haze, may have masked the colours. I think it, therefore, probable that Mr. Burnham, who has taken a good deal of trouble in the matter, is correct in his suggested identification, at least so far as my own observation is concerned. I may add that I have on several occasions, since the re-mounting of my telescope, looked in vain for a comes to ω^1 Cancri, and have been inclined to suspect that the small star seen by me in 1872 may have been a spurious image caused by an iron rod which crossed my shutter opening, though I am generally on my guard against deceptive appearances arising from this cause.

The opening sentence of Mr. Burnham's letter would seem to imply that Mr. Buckingham communicated the announcement of the duplicity of ω^1 Cancri to the Royal Astronomical Society. This was not the case. Mr. Buckingham merely mentioned the star in casual conversation with myself, but without giving any particulars of his observation, so far as I can recollect.

Cuckfield: March 15, 1878.

GEORGE KNOTT.

FRACASTORIUS.

In the *Astronomical Register*, for December, 1877, attention was called to IVBp6, an object north-east of Fracastorius, now it is intended to mention some objects upon the floor. Before doing so, however, it will be as well to point out a mistake in the last: for "northwards" p. 331, line 10, read "southwards." Noting Mr. Birt's remarks upon p. 27 of the January *Register*, it may be added that the western boundary of the depression was marked by a bright side. When the terminator was passing through the depression on 1878, January 8, 5h, there was just the irregularity at that point which would be caused by a depression extensive, though very slight in depth. In a sketch dated 1870, July 17, Ingall shows two small craterlets which he designates *a*, rather east of the centre of the floor. Gaudibert, in a figure in No. 588 of the *English Mechanic*, p. 405, the

result of observations on 1873, April 2; 1874, April 21; and 1876, April 28, places a mountain in this position, elongated north and south, its southern end dividing into two short branches, giving it a somewhat Y shape. Neison (map xix.) gives a single craterlet in place of the mountain, and which he marks z. In a drawing by Durrad, 1876, April 25, a mountain is shown, throwing a tapering shadow of about its own length. Birt has "a note that on June 27, 1876, I (Birt) saw in its locality with Mr. Barclay's (Leyton) 10in. refractor, a neat craterlet," the terminator lying through Eudoxus and Aristoteles, also just east of Kant. On August 8, 13h, and December 4, 13h, 1876, I have the place marked by a hillock. On 1877, May 17, 9h, I found an irregular square-shaped mountain—a "square" with rounded corners and concave sides—the north-east corner rising into a peak (possibly this "peak" may have been a craterlet). On 1877, June 29, 14h, the hillock was again seen as on 1876, December 4. In my figure of 1877, September 26, 11h, this point is again shown, having two other hillocks much smaller west and south-west of it, both within the confines of the "irregular square." The peak is registered IVBx 10. The objects to which it is wished to direct attention must now be pointed out. They are ridges, mounds, or undulations upon the floor. In the *Astronomical Register* for November, 1876, Mr. Birt mentions the discovery by Durrad of a ridge extending from the central hillock (IVBx 10) to the object (xlii. of the synopsis), which Mr. Birt has termed a "round topped tableland." On April 17, 8h 45m, 1877, Simms found two ridges, "both extending from the central mountain, one in a north-eastward direction, and the other in a north-westward direction, the former terminating at the northern extremity near the angular point in the north border of Fracastorius, immediately south of the shallow crater XI. of Mr. Birt's synopsis, and the latter near the small mountain" situated about midway between IVp 9 and the "round-topped tableland." In my sketch of 1877, September 26, 11h, six ridges, or rather low mounds, are shown. I. (8) This is Mr. Simms' second one, it extends from a small craterlet marked f in the sketches of Gaudibert and Ingall, about three-quarters of the distance towards IVBx 10, its direction pointing directly towards Gaudibert's 19, between the "round-topped tableland" and IVBp 9. II. (Q), This is Durrad's ridge, it is parallel to, and north-west of 8; it extends from the "round topped tableland" (which by-the-by appeared to me, 1877, September 24, 10h, as a bright spot which gave me the idea of a cavity filled with vapour, from the centre of which a bright and glistening hillock rose), to nearly midway between the "tableland" and the northern of the two small hillocks west of IVBx 10. III. (P), West of Q and parallel to it, also about equal in magnitude. IV. (M), Which preserves the same parallel (N.W. to S.E.) direction, having its northern extremity south of the southern extremity of Q, in the same meridian. V. (N), is smaller, and lies west of the northern extremity of M, and the southern end of P; and VI. lies south-east of IVBx 10, on a line drawn along 8, through IVBx 10, and produced. The most singular fact is that all these six mounds preserve the same parallel. On 1877, June 29, 14h, I strongly suspected the existence of two other ridges on the southern floor, one from IVBx 10 directly south to the hillock which terminates the terrace under the southern wall; and the other from a point a little west of IVBx 10, towards the south-west, ending at a hillock marked n in my figure on p. 480, vol. xxiv. of *English Mechanic*. Verification of these details would be useful.

FRANK C. DENNETT.

Erratum in April No., p. 106, line 4, for $1\ E\ \beta^1$ read $1\ E\ \theta^1$.

ASTRONOMICAL OCCURRENCES FOR MAY, 1878.

DATE.		Principal Occurrences.	Jupiter's Satellites.	Meridian Passage.
		h. m.	h. m. s.	h. m.
Wed	1	Sidereal Time at Mean Noon, 2h. 37m. 7 ^s 37 ^s .	1st Sh. E. 14 11 1st Tr. E. 15 31	Spica. 10 39.9
Thur	2	• New Moon Conjunction of Moon and Mercury 4° 4' S.		10 36.3
Fri	3	Sun's Meridian Passage 3m. 15 ^s 19s. before Mean Noon		10 32.1
Sat	4			10 28.1
Sun	5	Conjunction of Moon and Mars 2° 28' S. Conjunction of Saturn and Venus 1° 13' N. Saturn's Ring: Major axis = 36".57 Minor axis = 2".20		10 24.2
	6	Near approach of α Geminorum (3 $\frac{1}{2}$) Transit of Mercury Inferior conjunction of Mercury and Sun		10 20.3
Tues	7			10 16.4
Wed	8		1st Sh. I. 13 45 1st Tr. I. 15 3 1st Oc. R. 15 48 1st Sh. E. 16 4	10 12.4
Thur	9	• Moon's First Quarter	1st Oc. R. 14 34	10 8.5
Fri	10			10 4.5
Sat	11			10 0.6
Sun	12			9 56.7
Mon	13			9 52.7
Tues	14			9 48.8
Wed	15	Uranus at quadrature with the Sun Illuminated portion of disc of Venus = 0.561 Illuminated portion of disc of Mars = 0.957	1st Ec. D. 12 53 32 4th Sh. I. 14 8 1st Sh. I. 15 38 1st Tr. E. 16 2	9 44.9
Thur	16	• Full Moon Occultation of γ Scorpii (6) Reappearance of ditto Near approach of π Scorpii (3)	1st Ec. D. 12 51 55	9 40.9

DATE.		Principal Occurrences.	Jupiter's Satellites.	Meridian Passage.
Fri	17	Sidereal Time at Mean Noon 3h. 40m. 12 ^s 27 ^s .	2nd Tr. E.	h. m. s. h. m. Spica. 9 37 ^o 0
Sat	18	Sun's Meridian Passage 3m. 47 ^m 66 ^s . before Mean Noon	1st Tr. E.	13 42
Sun	19			9 29 ^o 1
Mon	20			9 25 ^o 2
Tues	21	11 Conjunction of Moon and Jupiter 0° 24' N.		9 21 ^o 3
Wed	22	14 34 Near approach of 42 Capricorni (5) 14 41 Occultation of 44 Capricorni (6) 15 17 Reappearance of ditto	3rd Sh. E. 2nd Ec. D.	14 49 15 27 16 9 17 ^o 3
Thur	23	13 41 c Moon's Last Quarter	1st Ec. D.	14 45 49 9 13 ^o 4
Fri	24		2nd Tr. I. 2nd Sh. E. 1st Tr. I. 4th Oc. R. 1st Sh. E. 2nd Tr. E. 1st Tr. E.	12 17 12 31 13 11 14 0 14 19 15 13 15 31 9 9 ^o 5
Sat	25	Saturn's Ring: Major axis=37 ^o 56 Minor axis= 2 ^o 75	1st Oc. R.	12 44 9 5 ^o 5
Sun	26	13 13 Occultation reappear- 4 ance of 51 Piscium (6) Conjunction of Moon and Saturn 6° 22' S.		9 16 ^o
Mon	27			8 57 ^o 7
Tues	28	0 Conjunction of Moon and Venus 7° 23' S.		8 53 ^o 7
Wed	29	19 Conjunction of Moon and Mercury 9° 8' S.	3rd Sh. I.	15 17 8 49 ^o 8
Thur	30			8 45 ^o 8
Fri	31	13 47 • New Moon	2nd Sh. I. 1st Sh. I. 2nd Tr. I. 1st Tr. I. 2nd Sh. E.	12 32 13 53 14 44 15 0 15 27 8 41 ^o 9
JUNE				
Sat	1		4th Sh. E. 1st Oc. R.	12 28 14 33 8 38 ^o 0

THE PLANETS FOR MAY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	3 48 16	N.18 54½	11".4	0 27.1
	9th	2 46 46	N.15 4	12".0	23 34.2
	17th	2 37 55	N.12 25	11".0	22 53.9
	25th	2 45 13	N.12 7	9".6	22 29.8
Venus ...	1st	23 45 8	S. 2 15	24".0	21 4.5
	9th	0 15 22	N. 0 19	22".0	21 3.3
	17th	0 46 35	N. 3 8	20".2	21 2.9
	25th	1 18 44	N. 6 5	18".8	21 3.5
Jupiter ...	1st	20 35 9	S. 19 4	37".4	17 55.1
	9th	20 37 9	S. 18 58½	38".4	17 25.6
	17th	20 38 21	S. 18 55½	39".4	16 55.3
	25th	20 38 45	S. 18 56	40".4	16 24.3
Uranus ...	1st	9 51 27	N.13 48	4".2	7 13.1
	13th	9 51 39	N.13 46½	4".0	6 26.2

Mercury may be observed for three-quarters of an hour after sunset at the beginning of the month, the interval rapidly decreasing. Towards the end of the month he will be visible before sunrise.

Venus rises an hour and twenty minutes before the sun on the 1st, the interval slightly increasing.

Jupiter rises an hour and a half after midnight on the 1st, the interval decreasing. On the last day he rises before midnight.

TRANSIT OF MERCURY.

There will be a transit of Mercury across the sun's disc on May 6th, 1878, partly visible at Greenwich.

	G. M. T.	h.	m.	s.
External contact at ingress, May 6	...	3	12	25
Internal contact at ingress	...	3	15	32
Least distance of centres 3' 47".4	...	7	0	4
Internal contact at egress	...	10	44	34
External contact at egress	...	10	47	43

Angle from { contact at ingress 45° towards the E. } for direct image.
 North Pole of { contact at egress 100° towards the W. }

JUPITER.

1878 oh. Gr.	Angle of position 2½'s axis.	Latitude of Earth Sun above 2½'s equator.		Equat. diameter.	Defect of illumination.
May 5	343.62	—0.68	—1.04	39.96	0.38
15	343.45	.63	1.00	41.27	.37
25	343.39	—0.59	—0.95	42.60	0.33

The assumed First Meridian passes the middle of the illuminated disk at the following times :

1878.	G. M. T.				
	h. m.		h. m.		h. m.
May 1	1 22'4	May 11	9 28'4	May 22	3 29'4
	11 17'9		19 23'8		13 24'8
	21 13'3	12	5 19'3		23 20'2
2	7 8'8		15 14'7	23	9 15'6
	17 4'2	13	1 10'1		19 11'0
3	2 59'7		11 5'5	24	5 6'4
	12 55'1		21 0'8		15 1'8
	22 50'5	14	6 56'4	25	0 57'2
4	8 46'0		16 51'8		10 52'6
	18 41'4	15	2 47'2		20 48'1
5	4 36'9		12 42'7	26	6 43'5
	14 32'3		22 38'1		16 38'9
6	0 27'7	16	8 33'5	27	2 34'3
	10 23'2		18 28'9		12 29'7
	20 18'6	17	4 24'4		22 25'1
7	6 14'1		14 19'8	28	8 20'5
	16 9'5	18	0 15'2		18 15'9
8	2 4'9		10 10'6	29	4 11'3
	12 0'4		20 6'0		14 6'7
	21 55'8	19	6 1'5	30	0 2'1
9	7 51'2		15 56'9		9 57'5
	17 46'7	20	1 52'3		19 52'9
10	3 42'1		11 47'7	31	5 48'3
	13 37'5		21 43'1		15 43'7
	23 33'0	21	7 38'5	June 1	1 39'1
			17 34'0		11 34'5
					A. M.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN
MAY, 1878.

By W. R. BIRT, F.R.A.S., P.S.S.

The following recorded observations were made in 1861, May 18, 7. 0, the moon's age being 8.87 days.

In the present month, May 1878, the moon's age at noon on the 11th will be 9.0 days; during the evening of that day, should it be clear, the objects specified below may be observed. A notice of whatever may be seen if sent to Buckhurst Hill will meet with due attention.

Tycho half in shadow; terminator some little distance from it to the east. Gamicus and Pitatus, both west of the terminator, very distinct. The central mountain in Tycho very magnificent, its shadow touching the base of the interior eastern rim; it then passes through Copernicus, the floor of which is in shadow and the interior of the eastern rim beautifully illuminated. The group Parry, Bonpland and Fra Mauro between Pitatus and Copernicus well brought out, being west of the terminator as well as Le Verrier, Pytheas and Lamhart.

The short lateral mountain range on the north-west of Plato has two well defined peaks on the east side casting well defined shadows, the land rises a little on the west, inclosing between it and the east range a somewhat narrow shallow valley which occasions the break in the north-west rim so very frequently observed.

[The Rev. R. S. Hutchings, of Salisbury, observed the two peaks mentioned above in March, 1878, viz., on the 13, 14 and 16, he also observed the western boundary of the crateriform depression north of Plato, W. R. B.]

Western rim interior shadow well marked, irregular with three projecting points corresponding to B. and M.'s γ , δ and ϵ very distinct. On the south-west the shadow is a mere line inclosing two bright spots which appears as two small mountains within the rim.

South-west exterior. The ravine Z in my key-plan very distinctly marked, the south-west slope of the rim east of the ravine very luminous, indicating its greater altitude. Note.—The sun is shining very obliquely on the highest parts of this ridge. Query, Are the two bright mountains, mentioned above, on the interior ledge or terrace observed at later epochs of illumination?

[Miss Ashley, of Bath, observed the ravine between March 13 and 24, 1878. She says, "there is a sort of ridge or double line of the wall of Plato, beginning just south of the crater G, and going to a point north-east of B. and M.'s γ . W. R. B.]

Floor tint a greenish grey. The second of B. and M.'s stripes from the east rim distinct but very faint.

At 8 p.m. of the same evening, May 11, 1878, moon's age 9.33 days, the following objects observed, 1861, March 20, 10. 25, moon's age 9.34, may be seen.

Terminator a little eastward of the eastern edge of Copernicus, which is a very magnificent object and grazing the eastern edge of Clavius.

North interior brightly illuminated from the mountain chain on the north-west.

East the rock ζ very brilliant, its shadow being eastward of Plato, and being exceedingly well marked.

South interior well illuminated.

South-west exterior. The ruggedness of the exterior slope well seen with the ravine Z running nearly parallel with the summit. The rock λ south-west of Plato very distinct.

North-west. The ruggedness of the exterior slope very distinct, also the rock γ , north-west of the crater Plato.

South-west, west and north-west interior in shadow, the shadows short.

Floor a nearly uniform tint, bluish grey, the dash of blue faint but decided, the colour of the floor darker than that of the Mare Imbrium.

As the observations of the surroundings of Plato in connection with the Selenographical Society are proceeding vigorously, notices of any of the above or any other objects near Plato will be valuable.

Buckhurst Hill: April 16, 1878.

Books received.—Ueber Schmidt's Neuen Stern im Schwan. Von Dr. Osw. Lohse. Berlin. 1878.—Popular Astronomy. By S. Newcombe, LL.D., &c., &c. London: Macmillan & Co. 1878.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To March, 1878.

Barneby, F.
Jackson-Guillt, Mrs. H.
Stothard, Dr.

To April, 1878.

Box, A. M.
Vernon, G. V.

To Dec., 1878.

Backhouse, T. W.
Green, N. E.
Harker, J. C.
Lancaster, J. L.
Lancaster, W. L.
Parnell, J.

To March, 1879.

Vines, D.

The *Astronomical Register* is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings per Quarter, payable in advance**, by Penny postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, Angel Court, Throgmorton Street, E.C., not later than the 15th of the Month.

The Astronomical Register.

No. 186.

JUNE.

1878.

ROYAL ASTRONOMICAL SOCIETY.

Session 1878—79.

The Third Meeting of the Session, May 10th, 1878.

Lord Lindsay, M.P., *President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

Maxwell Hall, Esq., M.A., Jamaica.

James L. McCance, Esq., Rathfern, Rayner's Road, Putney Hill, S.W.,

were balloted for and duly elected Fellows of the Society.

Mr. Glaisher announced that 64 presents had been received since the last meeting. Amongst these he drew special attention to several valuable books presented to the Society by Dr. Radford, and also to several volumes of the works of M. Flammarion, which had been presented to the library by the author on his election to the fellowship of the Society.

Mr. Ranyard read a paper by Mr. Waterston *On the heat of the stars*, after which

The Astronomer-Royal said: I am anxious to express to the Society, partly in consequence of some papers which have appeared in the *Monthly Notices*, on what might be expected to be seen during the transit of Mercury, and partly in consequence of what has been observed, my opinion on the spots of light which are said to be seen on the disc of Mercury during the transit. The point I wish to call attention to is this: it is my belief that these things are to be looked upon with great suspicion; and I have been led to this conclusion by observations of various kinds, I

VOL. XVI.

cannot say exactly relating to this, but bearing some relation to it; and which lead me strongly to look, in the first instance, to such things as reflection from the various surfaces of the lenses of which the eyepiece is composed, to reflection from the cornea of the eye, and reflection from the tubes of the eyepieces. I may mention a matter that occurred to me a great many years ago. In a room in the Observatory where I was accompanied by my friends (scarcely known to many members of the Society now), Mr. F. Baily and Admiral Smyth, both very accurate men in their way, we were looking out of a western window in the afternoon and admiring the prospect, when Admiral Smyth broke out with, "Why, there is a double star!" Mr. Baily looked at the same place, and said, "Yes, there is a double star." I looked at it, and there I saw it without doubt. I do not know whether they were more satisfied with the evidence than I was, but I thought it incumbent upon me to look a little further, and after a time I found that the origin of this double star was a bright spot on the cornea caused by the sun which was shining at the time. The window through which we were looking was of plate glass, of which the two surfaces were not perfectly parallel, and one gave a reflection which was the cause of one star, and the other gave a reflection which was the cause of the other star. Since then, at the time when there was a controversy concerning the origin of the prominences in solar eclipses, an astronomer came to me and said he had found the prominences and could exhibit them at any time; he showed them to me, and I saw them and scrutinised them a little, and found they were entirely caused by reflections from the interior of the eyepiece tubes. On a third occasion an astronomer reported to me that he had discovered that one of the exterior planets was accompanied by a luminous atmosphere. I warned him to be a little cautious about it, and to examine everything very carefully. I do not think he was quite persuaded by my cautions at first. At last I took it into my own hands, and examined the appearance which he described. I drew the eyepiece out of the tube of the telescope, and put black paper and other papers along the eye tube, and found most indisputably that the appearance of an atmosphere resulted from reflections from the interior of the telescope. After these experiences I need not say that I distrust every observation which speaks of a bright spot on the surface of Mercury. I need scarcely add that in the various successive transits of Mercury at distant times, which, so far as we know, have no relation to its time of rotation, it is exceedingly improbable that the same spot should appear in the centre of the disk during the different transits; and altogether I think I may invite the

Society, when they receive accounts in which are recorded bright spots on the surface of Mercury during the transit, to regard those accounts with a little caution, and put their own interpretation upon them.

Mr. Christie said after the remarks of the Astronomer-Royal he must be cautious in bringing forward the observations he was enabled to make of the transit. The observations were made in a very limited time, through the intervals of cloud, and he simply recorded what he saw without trying to explain the causes. He saw a minute bright spot in the middle of Mercury, which as far as he could make out did not depend on the motion of his eye. It remained apparently in the same position, but he had no opportunity of testing that point thoroughly. He also saw a ring of light around the planet. It might have been an effect of contrast. It seemed of irregular shape, not altogether circular, and in the interior of the larger ring (which was perhaps half the radius of Mercury in breadth) there was a brighter ring, perhaps a second in breadth, that seemed perfectly circular. The outer ring should perhaps rather be called a halo than anything else. He observed the transit with five different eyepieces, *i.e.*, he used both positive and negative eyepieces in five different ways—first, an ordinary negative eyepiece with and without a solar diagonal and Barlow lens, and then two forms of polarizing solar eyepiece. The light was consequently viewed after several reflections, from glass surfaces, and he used also a neutral-tint wedge. He could not pretend to have thoroughly investigated the phenomena, as he had only ten minutes at his disposal. Capt. Tupman was also engaged in viewing the transit, and he would read his account of it, which was something a little different from his own. Capt. Tupman reported that he scrutinised the planet with the powers of 110, 140, and 240, but he could not detect any permanent white spot within the disc; there was occasionally a diffused light over the planet's disc, and he could not detect any ring, luminous or shadowy, surrounding the limb; he was struck with the extreme sharpness of the planet's outline, but the clouds made further observations impossible. These observations, Mr. Christie remarked, were directly opposed to his own. There were, however, fourteen other observers within the grounds of the Observatory, and some of them saw the white spot while others did not, and some saw a bright ring and others did not.

The President said: We are favoured this evening with the presence of a distinguished American astronomer, Mr. Rutherford, of New York. The meeting will, I am sure, be glad to hear any observations he may have to make on the question raised by the Astronomer-Royal.

Mr. Rutherford: From my experience, and from the internal evidence afforded by the records of the observations, I should think that the Astronomer-Royal is entirely right. If one gentleman sees a dark ring, and another a bright one; and if one sees a bright spot, and another no spot at all, it seems to me the explanation is to be found in the instrument; and it is quite possible, as the Astronomer-Royal has well said, that many reflections take place from such a brilliant object as the sun. One may remember that the image of the sun made by a small lens, such as the lens of an eye-piece, would be a small point. I have used the image of the sun made by such a small lens as a star for testing microscopic objectives, and for that purpose it makes an excellent and brilliant star. The whole surface around the image of Mercury would contribute to make this little star, and any idiosyncrasy of the inclination of the tubes would determine its place upon the disk; and, therefore, I think that, all things considered, there can be little doubt that the Astronomer-Royal's explanation is the right one.

Mr. Dunkin read a paper on his observations of the transit of Mercury. He said that there were thirteen or fourteen observers within the grounds of the observatory, and five or six of them made special observations, accounts of which were now presented to the Society. These reports had been sent in to the Astronomer-Royal the day after the transit, without the observers having in any way communicated with each other. His own observations differed considerably from what had been seen by others. When he first saw Mercury it had advanced some distance from the sun's limb, and was partly obscured by clouds. When the clouds cleared both sun and planet were superbly defined, in fact he had never seen an image of the sun better defined. The observations were made with a telescope by Simms, of six inches aperture, that had been used for the transit of Venus. Mercury appeared intensely black, except for a quarter of a minute, when he saw a point of light near the centre a little towards the following limb of the planet. There could be no doubt of this phenomenon, though it was only seen occasionally. After an interval of two minutes the point of light became more visible, but a passing cloud soon obscured it. During the few minutes of superb definition a ring or corona of light around Mercury was visible, its breadth he estimated at about a third of the diameter of the planet. To his eye this corona was fainter than the sun, and appeared with radiating arms. Around the intensely black disk of Mercury this halo seemed to be of a half tint, and his impression was that it was produced only by contrast. The halo was perfectly uniform.

The President thought it would be better to receive all the papers on the subject of the transit first, and take the discussion on all of them after.

Mr. Proctor said that he had observed the transit with his little daughter. On looking into the telescope she at once saw the bright spot at the centre of the planet; her first impression was that her father had been playing a trick upon her, and had placed, as she thought, a circular piece of blackened card upon the eye-piece. The white spot she described as looking like the mark left at the centre by the leg of a pair of compasses which had been used to strike the little circle. He had himself seen the spot, and noticed that it changed in brightness and grew darker when the clouds passed over a part of the sun. If it were a real phenomenon it should, he thought, have grown relatively brighter, when the eye was sheltered from the sun's light; but it became fainter, and, as the Astronomer-Royal said, was due probably to the reflection of the sun's light in the eye-tubes. When he used a higher power he could not see the bright point, but he detected a misty sort of light in the middle of the disk. One point he was doubtful about. As the light of the point waxed and waned it seemed as though it assumed a triangular form. This appearance was quite momentary, and he did not like to speak of it with certainty. With regard to the ring, he did not think it was subjective, but real. It seemed to him a bright ring. He saw it on the side farthest from the limb, but when he directed his attention to the other side he could not see it. It was a very thin thread of light, and inside towards the limb it seemed to him to be dark. The limb was beautifully defined; in fact the definition was absolutely perfect.

Mr. Chambers said that at the point from which he observed the sky was clouded nearly the whole time; but half an hour before sunset the sun appeared through the clouds for ten minutes; and he had a glimpse of the planet. He could detect no indications of a spot, but a ring was visible around the planet; the breadth of the ring was slightly more than one diameter of the planet, though he had no chance of making measures. His wife, who saw it, described it as a dark ring; but, seeing it through clouds, he could not feel quite sure whether it ought to be described as bright or dark. He was struck with the extreme blackness of the planet, and the way in which it appeared detached from the sun itself.

Mr. Brett said he observed the transit at Putney, with his friend, Mr. Walter Pye. The contacts were lost by reason of an impenetrable mist. The planet was first seen after it had passed on to the sun's limb a distance nearly equal to its own

diameter, and it was afterwards seen at intervals through more or less dense cloud. The planet appeared as a black disk surrounded by a ring of bright light, and the black disk never appeared without the bright ring. His impression at first was that the width of the ring was one-third of the diameter of the black disk. To Mr. W. Pye the bright ring appeared not to be concentric with the black disk; but at the next break in the cloud this excentricity was not seen. They had good views of the planet and ring for about half an hour, during which time the definition was sharp, but at all times when the disk of Mercury was visible the bright ring was conspicuous; and his assistant observer confirmed this. Since the transit he had read Mr. Huggins' account of the transit of 1868, and his impression of the ring agreed with that account; it was equally bright up to its outer edge. Mr. Huggins, however, described it as a little brighter than the sun, whereas he saw it very much brighter, perhaps twice as bright. He desired to point out that his theory of specular reflection, by which he proposed to account for the bright ring around Venus, was strikingly exemplified by this transit of Mercury. The greater light on the limb of Mercury must evidently be taken from somewhere, and his hypothesis, that the globe of Mercury has a burnished surface covered by an envelope of great refractive power, explained the phenomenon of the ring.

The Astronomer-Royal asked whether the internal portions of the tubes of the eye-piece used by Mr. Brett were dead black or not?

Mr. Brett said they were most certainly black.

Mr. Ranyard: But even with a black surface at a very oblique incidence you may have a reflection.

Mr. Waters: Have these bright spots been observed on the dark spots of the sun? If it is an optical illusion, such spots or something corresponding to them should be observed with sun spots.

The President thought the discussion would be better deferred till all the communications had been received on the subject of the transit.

Captain Noble read a communication from Baron von Ertbon, of Antwerp, who observed the transit in company with M. de Boë and with some other Belgian astronomers. From 2h. 30m. to 3h. 45m. the sun was clouded, but the clouds cleared at about 4h. 30m., and at the instant of internal contact the limb of the sun was of the most extreme sharpness, such as is rarely seen. The internal contact was observed by M. de Boë at 3h. 14m. 14th 9s., G.M.T., and by M. von Ertbon one second later. None of the observers saw any black drop or ligament. The limb of the sun appeared

without any distortion, the observation being exceptionally fine. An aureola was seen surrounding the planet more brilliant than the surface of the sun, and there was a bright point on the surface of the planet. The width of the aureola was estimated as equal to the radius of Mercury, while the brilliant point had the appearance of the sun seen through a small hole. The observers saw the spot at some distance from the centre of the disk, rather nearer to the limb than the centre, while the English observers saw it at the centre of the disk. This Captain Noble suggested was a conclusive proof that, as Sir George Airy and Mr. Rutherford had remarked, the spot must be an optical phenomenon, and one which had no objective existence whatever.

Mr. Penrose said he was more fortunate than most of the English observers in having seen the ingress at Wimbledon. Towards the time of internal contact the sun's heat was even so great that it slightly scorched the glass wedge he was using. At 3h. 11m. 3'5s. he noticed, though he did not think it could be relied on, a little dark bar on the limb. Sixty-one seconds later there was a strongly indented notch. He watched the advance of the planet until it arrived at what he supposed to be the internal contact, when there seemed to be two little waves of light rapidly passing between the planet and what immediately became the continued outline of the sun's limb. He also noted 3h. 17m. 54'5s. as the time when the planet seemed to be one of its diameters from the limb, and then he saw clearly with a diagonal eye-piece a white light on the planet, but it was not central and was slightly flickering, and in the direction of the sun's centre. A lady who was with him saw it, but did not agree with him as to its exact position on the planet. It reminded him of the flickerings seen near a bright star. He did not see any ring around the planet, but an exceedingly hard black disk. The time he assigned to internal contact was 14m. 35½s. Greenwich time. The aperture of his telescope was 5¼.

Mr. Banyard read a paper from Mr. Cole, of Sutton, who gave the time of external contact 3h. 12m. 0s. and internal contact as 3h. 14m. 30s. No black drop was observed.

Mr. Glaisher read a paper from Professor Pritchard, who observed at Oxford. Dense clouds obscured the ingress, but twenty minutes after the commencement of the transit a break in the clouds occurred, showing a beautifully-defined image of the planet. During two glimpses he thought the centre of the planet's disk was brighter than the rest of it, but during the third glimpse he could not detect the phenomenon, but the appearance of a conspicuous brightness near the centre was confirmed by Mr. Jenkins, one of his assistants, but owing to the impossibility

of making experiments he should hesitate to say that the bright spot had an actual existence.

Mr. Ranyard said: We have another paper by Mr. Prince, but as he is not here I will give a short account of my own observations, which will not occupy many minutes. I observed the transit at Dun Echt with Lord Lindsay's 15-inch refractor which was stopped down to ten inches. As the planet came up to external contact the sky was covered with clouds, which at no time were sufficiently opaque to prevent the sun's limb being distinctly seen. From about ten minutes before the time of predicted contact, I observed the spectrum of the sky, outside the sun's limb, in the direction in which the planet was approaching. The spectroscopic with which I was observing was placed upon one of the finders of the larger instrument, the viewing telescope of the spectroscopic being turned so that the red end of the spectrum below C was in the field of view, the slit of the spectroscopic which subtended an arc of $5^{\circ} 45'$ upon the sun was swept outward across the path of the planet, but no variation in the brightness of any part of the spectrum could be detected. My reason for attempting this observation was that M. Janssen in observing the transit of Venus thought that by making use of a screen of blue glass he had been able to detect the body of the planet projected upon the background of the corona some time before it was visible without the screen. His idea was that the spectrum of the corona was rich in blue rays. He does not give the ground for this assumption, and the observations, as far as I am aware, point the other way. Stone, observing the spectrum of the corona during the eclipse of 1874, was of opinion that the continuous spectrum of the lower part of the corona was rich in red light, and the polariscopic observations also seem to point to the same conclusion. I therefore thought it better to examine the red end of the spectrum, but I could detect no variation in the brightness indicating the position of the planet. It must, however, be remembered that these observations were made through passing cloud. I continued my observations at the spectroscopic until some time after the signal for first contact had been given, but nothing indicating the position of the outer limb could be detected. I then passed to the larger instrument on which was a Mertz polarizing eyepiece with a power of about 400. The planet had now entered quite one-third of its diameter upon the sun's disk, but I was unable to see any traces of the outer limb of the planet. I then passed back again to the spectroscopic, and watched until the planet was two-thirds upon the sun's disk, but saw nothing to indicate the position of the outer limb. On turning again to the large instrument with the Mertz eyepiece, only a

small arc of the planet remained off the sun's disk, the definition had now greatly improved, but I could not see any trace of the part of the planet off the sun, and I certainly could not see any line of light round the planet as described by the transit of Venus observers. I watched the planet with the Mertz eyepiece, turning the field alternately brighter and darker, until the planet was entirely upon the sun's disk. I gave the signal for internal contact at the moment when I estimated that the two disks (*i.e.*, the disk of the planet and the disk of the sun) were in geometrical contact. Within a second, certainly not more than two seconds, after this signal, I saw a bridge of light separating the planet from the sun's limb. I saw no ligament and nothing that I could describe as a black drop. When Mercury was upon the sun's disk, I carefully looked for rings round the planet and for the spot of light upon the disk, but could detect no trace of either the one or the other phenomenon, but with a cardboard diaphragm of less than an inch in diameter upon the instrument used by Dr. Copeland, I thought that I saw a greyish ring round the planet, but did not feel quite certain as to its existence.

Mr. Common said he was fortunate enough to obtain a few minutes' observation of the transit with his 18-inch reflector. The disk of the planet appeared to him to be perfectly black, with a large hazy white spot rather to the south of the centre, and he suspected between the planet and the limb, a very minute dark spot, but he was unable to continue the observations owing to the clouds. He had another glimpse, when he tried whether he could project the spot on the screen of his photographic apparatus, but though he got a view for a moment he could not see any trace of the white spot, though the image of the sun was enlarged to eighteen inches, and the planet Mercury appeared about one-eighth of an inch in diameter.

Lord Lindsay said: For some time before the time of first contact, I was observing with a powerful spectroscope arranged in the manner devised by Secchi. Before the object-glass of a 6-inch telescope was a prism which broke up the image of the sun before it fell upon the slit, and behind the slit was an ordinary spectroscope, which built up a coloured image of the sun's limb from the rays of various refrangibilities passing between the jaws of the slit. I brought up the part of the sun's limb at which the contact of the planet was expected to take place to the part of the spectrum corresponding to the C line, and a needle-shaped projection corresponding to the C image of the chromosphere was visible, projecting a few seconds of arc beyond the limb of the sun. Clouds were passing across the sun's disk, and occasionally the whole limb of the sun appeared much obscured, but at

5h. 3m. 30s., that is 13s. before I observed the first indent of the planet's limb upon the photosphere, I saw the needle-shaped image of the chromosphere commence to get shorter, and it was gradually cut down by the body of the planet until exterior contact. As the planet's limb advanced upon the photosphere it appeared quite black with a definite edge. There was no bright ring round it, though I thought when the planet was quite upon the sun's disk, and I made use of a high power, that there was a decrease of darkness at the centre of the planet. This may have been an effect of contrast, but it had the effect of making the planet appear spherical. I spent some time in studying this shading off of light from the centre of the planet, and it appeared to me that the shading off was most marked when the aperture was decreased, but contracting the aperture—which I was enabled to do by means of an Iris diaphragm—I was unable to obtain any rings round the planet, though I looked carefully for them with the 6-inch with which I made the above observation of contact, with my 15-inch, which Mr. Ranyard had been using, and with a 4-inch, and with none of these could I see anything which could be described as a spot of light at the centre.

Mr. Ranyard: I omitted to say that after Mercury was on the sun, I placed the slit of the spectroscope across it, and tried carefully to examine the edges of the dark band, which the planet cut out across the field of the solar spectrum, but I could see nothing which indicated absorption in the atmosphere of Mercury. The observation is, however, of very little value, as the tremor in our own atmosphere was so great that the edges of the band were never sharply defined, and I could not satisfactorily bring the slit up so as to be tangential to the disk of the planet; besides this the spectrum of the diffused sun-light, dispersed by particles in our own atmosphere—which of course extended all across the disk of the planet—was sufficiently bright to have masked anything but very conspicuous absorption bands.

Mr. Huggins said as his observation of the transit of Mercury in 1868 had been referred to, he had better say that he had watched the bright point which he saw at the centre of the planet until the time of egress, and that at the moment the planet had passed half off the sun the spot instantly vanished, which appeared to show that the spot was obviously an optical and not a physical phenomenon. He had prepared an apparatus for the recent transit to take photographs of the spectrum of the sun with Mercury within the field of the spectroscope, but the sky was unfortunately clouded nearly the whole of the time, but when the sun came out clearly for two or three minutes he took a photograph, but whether Mercury was within the field of

the slit he could not say with certainty. Upon the photograph there was a dark band, the breadth of which corresponded with what would have been caused by Mercury. He was not yet able to say positively whether he might regard it as Mercury, but if so there was obviously no considerable modification of the solar spectrum. It would, however, be necessary to strengthen the photograph and make careful comparisons, before he could decide the point satisfactorily.

Mr. Christie desired to supply an omission in his former accounts. The ring or halo he saw was seen most distinctly with a small diaphragm, limiting the field of view to three diameters of Mercury, and it was seen more distinctly when the brightness was reduced almost to vanishing point by means of a polarising eyepiece. Making a black disk on a piece of paper he found he saw very much the same sort of halo round the black disc, and that the halo came out more distinctly when the dark glass was used to reduce the light, so that it was quite possible the outer halo was the effect of contrast.

Mr. Brett: The argument is that the weaker the image the more it will be affected by contrast.

Admiral Ommanney read a translation which he had made of a newspaper report of the observations of Baron Ertbon, and M. de Boë. The account appeared to be similar in effect to the paper read by Capt. Noble from the same source.

The President (in the absence of any comments on the papers) moved a vote of thanks to the authors.

Mr. Rutherford, at the invitation of the President, said: The small matters I have to bring before the Society are two-fold. One concerns these little plates, which are positive copies, taken from negatives of the sun made in my observatory in 1871. I caused them to be made a few days ago in New York on seeing M. Janssen's announcement that he had obtained photographs which showed granulations on the sun's surface—rice grains, or whatever your choose to call them. I had the pleasure of seeing one of M. Janssen's photographs in the possession of Professor Langley, an American astronomer. My purpose in bringing these photographs to the attention of the Society is simply to show that he was not exactly right in stating that the structure of the photosphere had never been before photographed, I do not claim that my photographs are equal to those of M. Janssen, but the granulations are quite visible. I bring them up, not for a moment to diminish the credit due to M. Janssen, but simply to make a small correction in the history of that part of astronomical photography.

I have here another matter. It is a photograph of the solar

spectrum, or so much as I find can be taken with a high power with ordinary collodion. It has been obtained with an interference plate, constructed by myself. The engine which drew the lines I constructed, and the interference plate with which this photograph was made would have about 17,300 lines to the inch. The photograph was taken in the spectrum of the second order, except at the extreme red, or rather the green end, for my spectrum only reaches to about E. There it was first taken in the first order and expanded afterwards to the same dispersion as the rest. I came away from New York rather unexpectedly, and had not time to put the letters of the lines upon them. I was agreeably surprised on putting a rule beside the photograph to find that the wave lengths corresponded almost to a scale of equal parts. Theoretically it should be so, but there are many things which conspire against this result. This is a print from twenty-eight different negatives. The negatives were first taken on a small scale. From these larger positives were made and then increased negatives; and then, of course, in the printing the paper is wet and liable to stretch, and in the copying there is room for errors to creep in; but I suppose the errors never amount to more than a millimeter and a half in the position of any line. Perhaps I may be permitted to say something of the spectroscope with which this was done, and to mention that I find the greatest assistance in doubling or trebling the focal length of my collimator. Everybody who is familiar with the spectroscope well knows that with a slit of a given aperture, if you double your collimator the angle subtending your slit is diminished one-half, consequently if you double your collimator you can open your slit twice as wide and get double the light. Then the form of condenser I used was also peculiar. It was simply an achromatic objective of a large sized opera glass combined with a lens which renders the rays parallel at their point of greatest condensation; so that I keep the image of the sun within as small dimensions as possible. The interference plate was drawn on speculum metal, which is the best material I have been able to find. Interference plates drawn upon glass and silvered give more brilliant spectra, but we have to deal then with two surfaces of glass, and it is difficult to make the two surfaces of glass parallel. In the case of speculum metal it is curious that these fine lines develop all the crystallization of the metal, but notwithstanding this the definition is quite fine; and I have not been able to find a substitute which will produce a better effect. (Applause.)

Mr. Ranyard: Is there any evidence upon the photographs with regard to the existence of the bright oxygen lines?

Mr. Rutherford: There are many lines upon the photographs brighter than the average background of the spectrum, but I have not confronted them with the oxygen spectrum, and Dr. Draper's photograph is on such a different scale that I have not been able to identify his lines on my print.

The President: I feel sure that in expressing our thanks to Mr. Rutherford I am only echoing the feelings of the meeting. He has given us a most interesting communication, and has really done magnificent work. I have never seen anything so fine as this photograph of the spectrum before.

Mr. Ranyard: Were these taken with short exposures?

Mr. Rutherford: With the very shortest exposures I could get, but different exposures for different parts of the spectrum.

Mr. Christie showed some drawings of nebulae which had been made by Sig. Tempel, of Florence. He remarked that these drawings, which were very carefully done, differed very materially from some of the published drawings, and they show that there is much left in this field still to be wished for. We needed a more accurate representation of what was seen, before we could rely on any hypothesis with regard to change in the nebulae. Sig. Tempel had made about 160 drawings, and had discovered about 50 new nebulae in the course of his researches without any systematic search, so that it would appear there were many more of these objects in the heavens to be picked up.

The President said he was acquainted with Sig. Tempel and the difficulties under which he laboured, his telescope having no clockwork, thus necessitating the moving of the telescope with one hand while he was drawing with the other.

Mr. Watts described a new form of observing chair, of which he exhibited photographs to the meeting.

Capt. Noble thought it was desirable that some new mode should be adopted of drawing nebulae. The present method showed them black on a white ground, which was just the reverse of what was seen in the heavens.

Mr. Christie said this difficulty had struck Sig. Tempel, who was not satisfied with the present methods of producing pictures of nebulae. He suggested that something might be done by lithography or steel engravings, or by photographs of drawings on fine paper.

Mr. Proctor suggested a method of printing with a greyish-white ink upon a black ground.

Mr. Ranyard said that he had tried to produce pictures of coronas by printing with a white ink upon black paper, but he had found that, though it succeeded very well for the faint parts, there was no ink which was opaque enough to produce the effect necessary for the brighter parts.

Mr. Proctor expressed a doubt whether this objection would apply to objects like the nebulae.

Mr. Howlett said that he should recommend a method of drawing in white by scraping away a black surface laid upon a white ground, and whereby very truthful representations may be made of both nebulae and comets.

The meeting adjourned at ten o'clock.

REVIEWS.

Results of the Astronomical Observations made at the Royal Observatory, Greenwich, 1875. (Extracted from the Greenwich Observations, 1875.)

Here we have: 1. Catalogue of 868 stars in R.A. and N.P.D. for 1875, January 1, with the usual accompaniments. 2. R.A. and N.P.D. of the sun, moon, and planets, deduced from the observations and compared with the *Nautical Almanac*, &c. This includes 57 of the minor planets observed at Paris and Greenwich. 3. Observations of γ Draconis with the reflex zenith tube, &c. 4. Observations of the solar eclipse, 1875, Sept. 28—29, with the equations deduced from the differences of R.A. and N.P.D. of the limbs and cusps. These last were observed with the great equatoreal, but some were also taken with the transit circle on the meridian. 5. Micrometer measures of the positions of Saturn's satellites, made with the great equatoreal. The satellites observed were Iapetus, Titan, Rhea, Dione and Tethys. 6. Eclipses, occultations, and transits of Jupiter's satellites compared with the *Nautical Almanac*, and occultations of stars by the moon, with the equations deduced, &c. In the eclipses the apparent errors in the *Nautical Almanac* were as follows:

I. disap. + om. 8'3s. I. reap. + om. 16'2s, by another observer — om. 6'5s. I. reap. — 2m. 52'5s. I. reap. — om. 8'6s. II. reap. + 1m. 43'7s, by another observer + 2m. 34'5s. II. disap. om. 50'2s. III. disap. — 1m. 32'6s. III. disap. — 3m. 43'6s.

7. Spectroscopic observations in 1874 and 1875, comprising (1) position angles and heights of solar prominences observed with the spectroscope. Some of these were made with a large spectroscope, having a train of 20 simple prisms, lent by Mr. Spottiswoode to the Royal Observatory. (2.) Motions of stars in the line of sight, concluded from measures of displacement of lines in their spectra. None of these, we imagine, are to be taken as final determinations, but the following are specimens of stars receding from us, with their motion in miles per second: Capella, 11; Rigel, 25; Sirius, 31; Castor, 61; Procyon, 34. A series of nine determinations ranging from -16.2 to -54.1 makes α Lyrae (Vega) to be approaching us at the rate of 41 miles per second. (3) Dark lines in the spectra of Aldebaran, δ Virginis and Mars, and corresponding elements. (4) Spectroscopic observations of Coggia's comet (III. 1874). (5) Results of spectroscopic observations of chemical elements. (6) Plate I. (coloured) Solar prominences, 1874, 1875, with horizontal and vertical scales. Plate II Spectra of carbon compounds and of nitrogen. And lastly (7) Measures of areas of spots and faculae upon the sun's disc on photographs taken with the photoheliograph.

Such are the contents of this invaluable work, prized, we doubt not, as it deserves to be, by all into whose hands it falls. It makes evident the increase of astronomical work of late years at the Royal Observatory—to

say nothing of other departments—for in 1873 the “Results” comprised 80 pp.; in 1874, 102 pp.; and this volume has 138 pp., of which the spectroscopic observations extend from p. 99 to p. 138.

The Universe of Stars: Presenting researches into and new views respecting the constitution of the heavens. By Richard A. Proctor. Second edition. Longmans. 1878.

This valuable work is a reproduction in chronological order of its author's papers on the constitution of the universe, from 1867 to 1873. It is profusely and finely illustrated. Amongst other plates we have two sections of the marvellous chart of 324,198 stars, which at first sight looks like various shadings produced by random touches of a fine pen, but examined with a magnifying glass each minute dot is seen to be round and distinct. The sublimest department of astronomy is discussed in these papers, and an entrance made hopefully into a field where observation and speculation will both find scope for many a year to come. It is one, too, requiring many observers, and in which amateurs may render important service, and Mr. Proctor points out how usefully their labours may be directed. Were we some twenty years younger, we should be glad to give ourselves almost exclusively to sidereal astronomy in its largest sense. Indeed few can now hope to advance the science who do not select a special branch, and stick to it with all the devotion of their ability, means, and leisure. Mr. Proctor's views, as we have before had occasion to observe, appear sound and convincing. Perhaps it is too much to expect from his versatile abilities, already engaged in so many lines, that he will be able individually to follow up to any great extent the vista he has thrown open, but he will certainly not be slow to utilise any future discoveries, which combined with such further reflection as he may devote to this at once the most grand and the most complex of astronomical studies, may in time lead to another volume of like interest with this one. Just in passing we would observe that having been some time occupied in mapping down, for the most part simply by careful eye-estimate, the stars in the clusters of the Pleiades, Hyades, and Præsepe, visible with a power of 60 on a $3\frac{1}{2}$ -inch refractor, we have been pleased by observing on a small scale the appearance of star-streams and star-sprays which are discussed in this work, of which in conclusion we enumerate the contents: Star streams—Star-streams and star-sprays—Notes on nebulae—A new theory of the universe—What fills the star depths—Star drift—Are there any fixed stars?—News from the stars—Essay on a chart of 324,198 stars—On two rich nebular regions—The construction of the heavens—Notes on star-gauging—Principles of star-gauging—Summary and conclusion.

A Star Atlas for Students and Observers, showing 6,000 Stars and 1,500 Double Stars, Nebulae, &c., in Twelve Maps on the Equidistant Projection; with Index Maps on the Stereographic Projection. By Richard A. Proctor. Fourth edition. Longmans. 1877.

This atlas deserves the extensive acceptance it has met with. The maps, 13 inches in diameter, are all that can be desired in fulness, clearness, and we doubt not accuracy. For ourselves, we should have preferred to have them in separate sheets, unbound, so as to admit of being hung up, or pasted on card-board for ready reference. As it is, their being in the actual binding, folded down the middle, seems to us rather a pity, though, certainly, it is the best way for portability. Many

corrections and additions, we learn in the introduction, have been made in this edition. Amongst others 60 or 70 stars, double or triple, a correction due to Mr. S. W. Burnham, of Chicago. The stars are taken from the B. A. Catalogue, and include all down to the sixth magnitude inclusive. Besides these stars the atlas shows:—1. All the nebulae down to the order "very bright" (inclusive), in Sir J. Herschel's Catalogue, and all Messier's. 2. All binaries in Mr. Brothers's Catalogue. 3. All the objects in the Bedford Catalogue. 4. All Schjellerup's red stars (293 in all). 5. All variables in a list of 136, by Mr. Baxendell, of Manchester. 6. All stars described as double or triple in Sir J. Herschel's Cape Observations are so marked. The constellation figures do not appear in these maps, and we think Mr. Proctor has done well to omit them, as the atlas is designed for practical working use. We also agree with him in his closing sentence: "I must admit that, so far as popular astronomy is concerned, I should be sorry to see the foolish old figures removed;" though we must take exception to the word "foolish," so far as the ancient figures are in question.

Transit of Venus. A popular account of past and coming transits from the first observed by Horrocks, A.D. 1639, to the transit of A.D. 2012; with 20 plates (12 coloured), and 38 woodcuts. Also an account of successes achieved in December, 1874, and suggestions respecting the transit of 1882. By Richard A. Proctor. Third edition, to which is added a note on the Results obtained by the British Expedition. Longmans. 1878.

It is gratifying to see this capital work in a third edition. In his account of the results of the British expeditions the author says, "I venture to predict that the British Halleyan observations, partial though they are, and only deriving value from combination with Halleyan observations by other nations, will be found to give results far more satisfactory than those obtained from the entire and exceedingly complete series of British Delisleian observations." (p. 239.) . . . From a series of calculations based on every other available method, it has lately become clear that the solar parallax lies not very far from $8''.85$, corresponding to a distance of 92,300,000 miles. All the best results gather closely around this value . . . Now the net result of the British expeditions . . . is to give a solar parallax of between $8''.75$ and $8''.76$, corresponding to a mean distance of 93,375,000 miles. This is far outside the limits between which all other results show that the true distance lies . . . to allow the new value to displace the mean estimate of former observations would be equivalent to according to the results a weight exceeding many times that assigned to the best work of many other astronomers." (pp. 240—41). These reflections seem just, and even if our own figures should eventually be supported by the results of the American and Continental observations, we apprehend they would still be received with mistrust in the present state of the question.

Annuaire de L'Observatoire Royal de Bruxelles. XLVine Année, 1878. Bruxelles. 1877. T. Hayez.

This compact little pocket volume amongst much useful matter contains a chronological table of discoveries in meteorology, which has been carefully compiled from many works and libraries. This we fancy has never been done before. Many items of interest are found in it. For

instance, we learn that Kepler was the first (as far as is known) who studied the regular forms of the crystals of snow, and published a treatise on them in 1611. Meteorology is a principal feature of this volume. In regard to our own science, we rejoice to find that a taste for astronomy has of late years been developed in Belgium. The geographical position of six private observatories is given, the names of places and astronomers being as follows: Aertselaer; Oct. Van Erthon, Anvers; Ad. de Boe, Gand; Dr. V. Monckhoven, Louvain; F. Terby, Malines; G. Bernaerts, Schaerbeek; Ch. Montigny (p. 132). M. Montigny has devised an ingenious and delicate instrument for observing the scintillation of stars as a prognostication of weather (pp. 239—241, 245—247). M. Terby is well known for his studies on the spots on Mars. Dr. Monckhoven is a regular observer of the solar protuberances, and was the only person in Europe to mark the appearance of the spot on Saturn, by which the American astronomers have determined the rotation of the planet. It is hoped that shortly M. Bernaerts will communicate his numerous drawings of the solar spots. Our readers will recognise M. Ad. de Boe as an occasional contributor to the *Astronomical Register*. The director of the Royal Observatory of Brussels is M. Houzeau.

Report of the Temple Observatory, Rugby, for the year 1877. pp. 12.

The principal work of the year has been the building of a new observatory, with a house near it for the curator or sub-curator, who has it free of all charge in consideration of his services. The cost of observatory and house, £1,234, was defrayed by subscriptions from the Masters, old Rugbeians, parents of boys now in the school, and other friends. After details of the construction, the instruments are described. 1. The equatorial, 8½-in., by Alvan Clark, which formerly belonged to Mr. Dawes, who considered it one of the most perfect instruments he had ever seen. Views of Mimas, of Neptune's satellite, and several double stars of excessive delicacy which had escaped scrutiny with the Dorpat telescope, 9·6-in., and the Pulkova, 15-in., attest the superior defining power of this glass, and the equatorial mounting is worthy of its excellence; the focal length is 108½ inches. It was purchased in 1871 by Mr. Wilson for 400 guineas. 2. Transit instrument, 2½-in. and 29 in. focal length, the gift of Mr. Edward Crossley, F.R.A.S. of Halifax. 3. Sidereal clock by Cooke & Sons, and mean time clock, both furnished with means for sending electric currents in order to control other clocks. 4. Reflector; the mirror by With, 12½-in., and 6½ feet focal length. It is equatorially mounted and driven by a clock. The whole, with the exception of the glass, was made in Rugby. It is chiefly used with the larger spectroscope. There is also a small 3-in. refractor, and a siderostat. The observatory is open for members of the school at certain fixed hours; but the few who really work at the subject are allowed to stay a much longer time. At these times the sub-curator, Mr. Percy Smith (and often also the curator), is always present, and assists members of the school in taking observations with the equatorial and transit. By the rules made by the head-master, Dr. T. W. Jex-Blake, for the Temple observatory, the curator (Mr. G. M. Seabroke), appointed by the head-master, has entire charge of the observatory, and if requested by the head-master gives instruction to selected members of the school in practical optics and astronomy. He also keeps an account of all original work done in the observatory, and of all work done by members of the school. He makes an annual report (to be printed) to the head-master, on the state of the observatory and instruments, the work done in it,

number of visitors, &c. The sub-curator (assistant, in fact) is also appointed by the head-master.

This observatory appears to have been very well planned and constructed. It is, as heretofore, mainly devoted to double star measures and spectroscopic work. Vol. 43 of the *Memoirs of the R. A. S.* contains the names of 398 double and multiple stars taken at the Temple observatory during the last three years; and the curator continues his investigations into the motions of stars in the direction of the line of sight by the spectroscopic method. Altogether, considering the origin of this institution, its educational value, its judicious organisation, the excellence of its instruments, and the observational ability which has already made such good use of them, very great praise is due to all who have contributed to its establishment. Many young persons have there the opportunity of learning something of astronomy. Some may there lay the foundation of future usefulness, and perhaps a few may in after years look back from a position of eminence as astronomers on the hours spent in the Temple observatory as those which formed or developed their taste for, and fixed their devotion to, the noblest of the sciences.

The report is accompanied by a description of the equatorial by Mr. Dawes, a letter on the subject from the same, and another from the maker, Mr. Alvan Clark; also Rules for the Temple observatory, by Dr. Jex-Blake.

Stonyhurst College Observatory. Results of meteorological and magnetical observations, 1877. Manress Press, Roehampton. 1878. pp. 56.

Amongst other work, Father Perry states in the introduction, that they are engaged in the reduction of very complete meteorological work done at Kerguelen during the transit of Venus expedition. It extends over the four summer months of November, December, January and February, and cannot fail to be of interest and value. Two papers on magnetic observations at Kerguelen, and on the voyage there and back have been read before the Royal Society. The observation of the phenomena of Jupiter's satellites and the measurement of double stars continues. Three very clear days favoured the search for the supposed planet Vulcan, and an uninterrupted watch was kept up, as a complete negative result might under the circumstances be of considerable value. The object-glass of the great equatorial being affected by considerable spherical aberration, is now in the hands of the maker, who is confident of being able to correct it. Work is also done for the Board of Trade, the American Government, and the Upsala and Manila observatories. The monthly results of observations are presented in a very clear and compact form. They are followed by a summary for the whole year, agricultural notes, observations of crops and flowers, of trees and shrubs; observations of upper clouds, barometer readings, maxima and minima, with monthly curves, and summary of the same for eight years with curves. Finally, monthly magnetic observations with notes on magnetical disturbances and storms; the greatest of which occurred in May, on the 11th of which month, besides variations in other particulars, there was an easterly movement of the magnet through $27^{\circ} 35'$ in 15 minutes, followed immediately by a westerly movement through $21^{\circ} 29'$ in 10 minutes. On the 28th the greatest disturbance of the year commenced, the lowest reading of the declination curve being on the 29th, when the diminution of the W. variation was $46^{\circ} 59'$. The westerly maximum occurred on the 29th at 7h. 40m. a.m., the total range since midnight

being 52' 31" for the declination. The report concludes with an extensive list of presents of books received from institutions and individuals at home and abroad. It is very evident that in Stonyhurst observatory, under the able direction of Father Perry, much valuable work is being done. We shall be glad to hear that the great telescope has been returned in perfect order.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

NEW NEBULA.

I wish to notice a nebula found on the occasion of observing 53 Virginis, which appears to be new as it is not in Herschel's general catalogue. About 40' southwest of 53 Virginis (2m. 20s. p. and 20' s.) is B. A. C. 4396, a star just visible to the naked eye (Heis 6-7 magnitude). The nebula is in the field with, and closely following this star. With the micrometer I found the position angle from the star to be $90^{\circ} 6'$, and distance 131" 10. The nebula is small but not very faint.

S. W. BURNHAM.

53 VIRGINIS.

Mr. Burnham's letter about this star led me to look over some old observations, made in 1859 with a telescope of 3 $\frac{1}{2}$ -inch aperture. I was surprised to find a note "very plain" against this star. MSS. of following years were accidentally destroyed, and I can find no other record till the years 1873, '74, '75, in all of which I could make nothing of this object beyond glimpses, and those by no means certain. At this distance of time I would not lay very much stress on the observations of 1859, though I think it most likely I was not mistaken. It might be well, however, to keep a watch on this star to detect a possible re-appearance.

G. J. WALKER.

COMES OF RIGEL.

Sir,—In the double-star observations made at the Temple Observatory, recorded in the newly issued volume of the *Memoirs* of the R. A. S., I notice the magnitudes of β Orionis are put down at 1.7. Smyth has termed this little companion of Rigel magnitude 9, being followed of course by Webb. I have always considered its magnitude considerably underrated, as I see it with comparative ease through a 2 $\frac{1}{4}$ -inch achromatic whenever the air is quiet, whereas the little companion of Polaris,

also rated 9 magnitude, is never satisfactorily seen through the same instrument, though further removed from its principal star. In Gore's interesting little work, "Southern Stellar Objects," the comes of Rigel is termed magnitude 8, but 7 is probably nearer the mark.

Upton Helions Rectory,

S. J. JOHNSON, F.R.A.S.

Crediton: Dec. 31, 1877.

ON THE PROPER MOTION AND POSSIBLE VARIABILITY
OF β CORVI.

Sir,—Smyth (*Celestial Cycle*, p. 271) gives two companions to β Corvi: one 7 m., P. 119° 5, Diff. R.A. 27° 45; and one 8 m., P. 306° 5, Diff. R.A. 28° 05. Lalande and Harding give two companions 8 m. and 9 m. *s. p.* In March, 1877, I looked at β with a 3-inch refractor (Browning) and found two companions, nearly in the same positions given by Smyth, but both fainter than Lalande's stars, especially the one *p.*, which Smyth rates 8 m., but which is now more like 10 m. If Smyth's position angles were correct, β must have a considerable proper motion towards the North-East, as it is now North of the line joining the two comites, whereas Smyth's position angles place it to the South.

With reference to the magnitude of β , Smyth says, "This is a fine star and has unquestionably the precedence of lustre in Corvus, which could hardly have been the case in Bayer's time, and what is singular it has no trivial Arabic designation. Ptolemy gave γ , or third degree of brightness to α , β , γ , δ and ϵ , but Tycho and Hevelius both rated α and ϵ of the 4th magnitude, and β has latterly been elevated to 2.3. Such discrepancies should be closely watched, for though the low altitude of the asterism may be against precision in this country, it must be recollected that Ptolemy, Ulugh Beigh, Alphonsus, and Piazzi had a smaller south polar distance."

Sir W. Herschel in 1783 found the order of brightness γ , δ , β , α . Heis rated β as 2.3 m. In March, 1876, and March, 1877 (in the Punjab) β seemed to me nearer 3 m. than 2 m. β is probably variable to a small extent.

I am, Sir, your obedient servant,

Dromard Rectory, Co. Sligo:

J. E. GORE.

Dec. 26, 1877.

LUNAR NOMENCLATURE.

Sir,—We have, by the help of "Haydn's Index of Biography" (authority *a*), and "Beeton's Dictionary of Universal Knowledge" (authority *b*), found explanations of a few of those names wanting explanations, in your list of lunar names concluded in your number for December.

The following six appear to us almost certainly the right ones:

Büsching (M.) *Anton Friedrich*. German geographer; published "Erd-beschreibung," d. 1793 (*a*).

Fra Mauro (M.). Italian geographer; made a planisphere in 1457; d. about 1459 (*a*).

Huhn (M.) *Philip Matthæus*. German mechanician; improved astronomical instruments; d. 1790 (*a*).

Pontanus (R.) *Johannes Isaak*. Dutch historian; d. 1639 (*a*)

Rocca (R.) *Angiolo*. Italian philologist; published "Della Comete" in 1577; d. 1620 (a).
Zagut (R.) *Zacuth, Abraham Ben Samuel*. Spanish Jew and astronomer; d. about 1520 (a).

The next two are merely suggestions—some other correspondent may perhaps be able to determine whether they are right.

Barocius (R.) *Barocci Frederigo* (?). Celebrated Italian painter; d. 1612

(b)
Rook Mts. (S.) *Rooke Laurence* (?). Astronomer and mathematician; published "Observations upon the Comet of 1652;" d. 1662 (a).

Of the doubtful names on your list we find something about the three following:

Geber (a) gives "d. about 765," and (b) "lived about the 8th century."

Gerard (M.). Is it not more likely to be *Gerard*, of Sabionnetta, Italian physician and astronomer; wrote "Theorica Planetarum;" lived in 13th century (a).

Hyginus (a) gives "flourished about B.C. 40."

Hoping these may be useful,

We are, Sir, faithfully yours,

Rathfern, Rayners Road, J. L. & J. McCANCE.
 Putney Hill, S.W.: Jan. 5, 1878.

NOTES ON SOME STARS POSSIBLY VARIABLE.

The following stars are probably variable, and merit further attention from observers:—

(1.) 66 Ceti. R.A., 2h. 6m. 37s., S. $2^{\circ} 57' 7''$ (1880). This star was rated 6 and $6\frac{1}{2}$ by Lalande, 7m. by Piazz, 8m. by Harding, and 6.7m. by Heis. In November, 1876, I found it about half a magnitude brighter than 63 Ceti (north of it) which was rated 6m. by both Harding and Heis.

(2.) π Ceti. R.A., 2h. 38m., S. $14^{\circ} 22'$. In January, 1876, I found this star slightly brighter than η Eridani, which Heis rates as 3 m. In November, 1876, it seemed slightly less than η Eridani.

(3.) ρ^2 Eridani. R.A., 2h. 56m. 48s., S. $8^{\circ} 9' 19''$ (1880). This star was rated 4 m. by Ptolemy, 5 m. by Suñi, Lalande and Harding, 6 m. by Argelander, and 5.6 by Heis. In Schjellerup's translation of Suñi's manuscript, its position is thus described: "C'est une étoile double, et la deuxième des quatre." The four stars alluded to seem to be ρ^3 , ρ^4 , η Eridani and Lalande 4966. In November, 1876, I found ρ^3 distinctly less than ρ^4 , which Heis rates at 6m.

(4.) τ^2 Eridani. R.A., 3h. 54m. 48s., S. $24^{\circ} 21' 17''$. This star was rated 4 m. by Ptolemy and Suñi, $5\frac{1}{2}$ m. by Lalande (7456), $4\frac{1}{2}$ by Lacaille, 5 Harding, and 4 m. by Argelander and Heis. Lalande's low estimate is worthy of remark, and renders it advisable to watch this star for variation. Ptolemy, Suñi, Argelander and Heis all agree in rating τ^6 and τ^2 Eridani as equal. Lacaille and Harding also rate both the same magnitude, but Lalande makes τ^2 , $1\frac{1}{2}$ mag. the fainter (!)

(5.) 64 Eridani. R.A., 4h. 54m. 20s., S. $12^{\circ} 42' 35''$. This star was rated 6 m. by Harding and Heis, and 63 Eridani the same magnitude.

In March, 1876, and December, 1876 (in the Punjab), I found 64 much brighter than 63, and nearly equal to α Leporis (5 m. Heis).

(6.) Lalande, 14088. R.A., 7h. 8m. 11s., S. $22^{\circ} 27' 48''$ (1877). Observed by Lalande in 1798, but no magnitude is assigned to it in the Reduced Catalogue. It is 9 m. in Harding's Atlas, but was rated 6 m. by Argelauder, in 1852. It is not in Heis' catalogue. In January, 1877, I found the star reddish and about half a magnitude *brighter* than the 6 m. Lalande 14105, closely south of it. In April and May, 1877, and January, 1878, it seemed a little *less* than the star to the south. The variability of this star may be considered as established.

(7.) Lalande, 38405-6-7-9. R.A., 20h. 0m., S. $4^{\circ} 46'$. It was rated 6, 7, $7\frac{1}{2}$, and 8 m. by Lalande, 6 m. by Harding, and is 6.7 m. in Heis' Catalogue. It is not in Argelauder's Uranometria. About $20'$ north, a little *p*, is the star Lalande, 38388-89-90 (6 and 7). On 31 August, 1877, I found it less than the 7 m. to the north; also less than a 6 m. (Harding) about $1^{\circ} 15' s. p.$ it. This latter star is Lalande, 38-214-15 (6 m., 7m.)

(8.) ρ Capricorni (11 Fl.). R.A., 20h. 22m., S. $18^{\circ} 13'$. This star was rated 5.6 m. by Heis. π Capricorni being rated 5 m. by the same eminent observer. In October, 1876, I found ρ slightly brighter than π . August, 1877, I found ρ *distinctly* brighter than π —about $\frac{1}{2}$ magnitude.

(9.) Lalande, 40411-12-13, Lacaille, 8621. R.A., 20h. 49m. 38s., S. $26^{\circ} 45' 13''$. This star, which lies about $1^{\circ} n. f. \omega$ Capricorni, was rated 6 m. by Lacaille, $5\frac{1}{2}$, $6, 7\frac{1}{2}$ by Lalande, 7 m. by Harding, and 6 m. by Heis. August 22, 1875, I found it about $6\frac{1}{2}$ m. and equal to a star between ω and ψ Capricorni, which Heis rates at 6 m. and identifies with B. A. C. 7205. August 30, 1877, I found it brighter than the star between ω and ψ .

J. E. GORE.

Dromard, Co. Sligo:
Feb. 4, 1878.

THE STAR LALANDE 31266-7.

Sir,—This star was rated twice as 1 mag. by Lalande (p. 353 of the *Histoire Céleste*). It is not in Harding's Atlas or Heis' Catalogue, but it is shown in Dien's Atlas (Flammarion Edition). With a binocular the only star I can see close to the place is a faint one about 10 mag. The star is possibly a remarkable variable. It lies closely *following* the $6\frac{1}{2}$ mag. star 31192 (Heis 6 mag. 5775 B.A.C.), and its place for 1880 is, R.A. 17h. 3m. 48s., N. $4^{\circ} 3' 55'' 56''$.

Ballisodare, Co. Sligo:
March 5, 1878.

Yours faithfully,
J. E. GORE.

P.S.—Mr. S. W. Burnham has kindly measured the above with (I presume) the Chicago refractor, under date of Feb. 26, and gives the following results:

$$\begin{array}{ll} \text{A and B} - P. = 309^{\circ}.2 & D. = 21''.35 \\ \text{B and C} - P. = 229^{\circ}.2 & D. = 11''.47 \end{array}$$

Belfast: March 12, 1878.

L. W. W.

LONGITUDE OF MADRAS.

The longitude observations undertaken by Captains Campbell and Heaviside, of the Great Trigonometrical Survey, during the past year, had for their object the determination of the exact difference of longitude between the Madras Observatory and that of Greenwich. The longitude of Madras has thus been found to be two seconds greater than that given in the *Nautical Almanac*. The work resolved itself into determining the difference of longitude between Madras and Bombay, between Bombay and Aden, and between Aden and Suez, the longitude of Suez having been carefully ascertained by the Transit of Venus party in 1874. The *Pioneer*, after describing the method adopted by these observers, remarks that Sir G. Airy, the Astronomer-Royal, considers it so satisfactory that he believes in the feasibility of a direct determination of the longitude of Melbourne, since there is a complete telegraphic connection between Melbourne and Madras, via Batavia and Singapore. The only question would be that of expense.—*Madras Weekly Mail*, 20th April, 1878.

METEOR.

March 15th, about 8h. 30m. a meteor of extraordinary size and brilliancy passed over Lahore. The light emitted in its passage was as bright as that of the moon at the full. It exploded to the westward, with a noise similar to thunder, and precipitated fragments in all directions.—*Homeward Mail*, April 8th, 1878.

The New Meteorological Observatory at Friesole is to be established on the top of the Seminario there, and will take a distinguished place amongst the sixty-four meteorological stations near the Alps and the Apennines, the observations of which are printed in the bulletins of the Italian Alpine Club. The members of the club are subscribing for its establishment. It will be furnished with the usual instruments. The Bishop Corsani, and the Rev. Prof. Nardi have warmly promoted this work. The Terrace of the observatory which commands a magnificent view in all directions, is 1,023 feet above the sea level.—From *La Nazione*, 20th June.

Padre Secchi writes in the *Osservatore Romano*, June 23rd, of the appearance of the sun on the previous day, shaded and spoiled of its rays, so that it resembled the moon with a greenish silver colour. "This phenomenon," he says, "is not rare when light dust from the African deserts clouds an atmosphere. In fact such dust appeared last evening and this morning in abundance on plants. * * * By telegraph we learn that at Naples also this atmospheric fog was experienced, but there has been only a moderate eruption of Vesuvius, and no fall of ashes there. The powder collected this morning at the observatory in plates, carefully cleaned and used always for this purpose, is of the usual brick-red colour, and is, moreover, mixed with filaments and granules of vegetable matter." The same appearance of the sun was remarked also at Florence at the same time by Countess Baldelli.

Books received.—American Journal of Mathematics, published under the auspices of the John Hopkins University. Vol. I. Baltimore. 1878. —Astronomical Observations made at the University Observatory, Oxford. No. 1. Oxford: Clarendon Press. 1878.

ASTRONOMICAL OCCURRENCES FOR JUNE, 1878.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Sat	1		Sidereal Time at Mean Noon, 4h. 39m. 29 ^s .63s.	4th Sh. E. 1st Oc. R.	12 28 14 33	α Coronæ 10 48.5
Sun	2		Sun's Meridian Passage 2m. 17 ^s .78s. before Mean Noon	1st Tr. E. 2nd Oc. R. 3rd Oc. R.	11 47 12 21 13 14	10 44.5
Mon	3	9 59 10 39 9	Occultation of Mars Reappearance of ditto Conjunction of Moon and Mars 1° 4' S.			10 40.6
Tues	4					10 36.6
Wed	5	8 54 9 48	Occultation of π ³ Can- cri (6) Reappearance of ditto			10 32.7
Thur	6	20 20	☾ Moon's First Quarter			10 28.8
Fri	7			2nd Sh. I. 1st Sh. I.	15 9 15 46	10 24.8
Sat	8			1st Ec. D.	13 2 24	10 20.9
Sun	9			1st Tr. I. 1st Sh. E. 3rd Ec. R. 3rd Oc. D. 1st Tr. E. 2nd Oc. R.	11 14 12 34 12 40 51 13 13 13 34 14 42	10 16.9
Mon	10		Near approach of 75 Virginis (6)			10 13.0
Tues	11					10 9.1
Wed	12					10 5.2
Thur	13	22 54	☾ Full Moon			10 1.2
Fri	14		Saturn's Ring: Major axis=38° 78 Minor axis= 3° 19 Sidereal Time at Mean Noon 5h. 30m. 35 ^s .80s.			9 57.3
Sat	15	14 15 15 26	Illuminated portion of disc of Venus=0.689 Illuminated portion of disc of Mars=0.975 Occultation of φ Sagit- tarii (31) Reappearance of ditto	1st Ec. D.	14 56 33	9 53.4

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.		h. m. s.		h. m.
Sun	16	10 17	Occultation of λ^1 Sagittarii (6)	1st Sh. I.	12 8	aCoronae
		11 31	Reappearance of ditto	2nd Ec. D.	12 25 35	—
		10 52	Occultation of λ^2 Sagittarii ($4\frac{1}{2}$)	1st Tr. I.	13 1	
		11 28	Reappearance of ditto	3rd Ec. D.	13 17 15	9 49'4
Mon	17			1st Sh. E.	14 28	
				1st Tr. E.	15 21	
Tues	18	18	Conjunction of Moon and Jupiter $0^\circ 11' N.$	1st Oc. R.	12 35	9 45'5
			Sun's Meridian Passage om. 47'37s. after Mean Noon	2nd Tr. E.	11 40	9 41'5
Wed	19			4th Tr. E.	14 31	
		13 57	Occultation of ϵ^1 Aquarii (6)			9 37'6
Thur	20	15 13	Reappearance of ditto			
		15 12	Occultation of B.A.U. 7986 (6)			9 33'7
		16 15	Reappearance of ditto			
Fri	21					9 29'8
Sat	22		☾ Moon's Last Quarter Conjunction of Moon and Saturn $6^\circ 40' S.$			9 25'8
Sun	23	18	Saturn at quadrature with the Sun	1st Sh. I.	14 2	9 21'9
				1st Tr. I.	14 46	
				2nd Ec. D.	14 59 34	
Mon	24			1st Ec. D.	11 19 17	9 17'9
				1st Oc. R.	14 21	
Tues	25			1st Sh. E.	10 50	
				1st Tr. I.	11 5	
				1st Tr. E.	11 32	9 14'0
				2nd Sh. E.	12 38	
Wed	26			2nd Tr. E.	14 1	
		23	Conjunction of Moon and Venus $7^\circ 23' S.$	4th Ec. D.	10 31 55	9 10'1
Thur	27			4th Ec. R.	14 46 37	
		13 42	Occultation of χ^1 Tauri (5 $\frac{1}{2}$)	3rd Tr. I.	9 52	9 6'2
		14 29	Reappearance of ditto	3rd Sh. E.	10 47	
Fri	28			3rd Tr. E.	13 29	
						9 2'2
Sat	29	14	● New Moon Conjunction of Moon and Mercury $2^\circ 39' S.$			8 58'3
Sun	30					8 54'4
JULY						
Mon	1			1st Ec. D.	13 13 38	8 50'4

THE PLANETS FOR JUNE.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	3 4 56	N.13 40½	8".2	22 21.9
	9th	3 41 13	N.16 49	6".9	22 26.7
	17th	4 32 10	N.10 31	5".9	22 46.0
	25th	5 38 10	N.23 30½	5".3	23 20.4
Venus ...	1st	1 47 40	N. 8 42	17".7	21 4.8
	9th	2 21 41	N.11 38½	16".7	21 7.3
	17th	2 56 51	N.14 25	15".7	21 10.9
	25th	3 33 18	N.16 55½	14".9	21 15.8
Jupiter ...	1st	20 38 25	S.18 58½	41".3	15 56.5
	9th	20 37 16	S.19 42	42".1	15 52.4
	17th	20 35 21	S.19 13	42".9	14 50.5
	25th	20 32 41	S.19 24½	43".9	14 16.4
Saturn ...	1st	0 8 43	S. 1 24	15".2	19 26.2
	9th	0 10 34	S. 1 14	15".4	18 56.6
	17th	0 12 4	S. 1 6½	15".6	18 26.6
	25th	0 13 12	S. 1 13	15".8	17 56.3
Uranus ...	2nd	9 53 3	N.13 38½	4".0	5 8.9
	18th	9 55 4	N.13 27	4".0	4 8.0

Mercury may be seen three-quarters of an hour before sunrise on the 1st, the interval increasing till the 20th, from which day the interval decreases.

Venus rises an hour and a half before the sun at the beginning of the month, the interval increasing to more than two hours on the last day.

Jupiter rises a quarter of an hour before midnight on the 1st, the interval increasing.

Saturn may be seen for two hours and a quarter before sunrise at the beginning of the month, the interval rapidly increasing. On the 30th he rises half-an-hour before midnight. He is at quadrature with the sun on the 24th.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN

JUNE, 1878.

By W. R. BIRT, F.R.A.S., P.S.S.

On the evening of the 10th June, 1878, moon's age between 9.6 and 10.0 days, the following objects recorded to have been seen in 1860, May 29, 9.50, and May 30, 8.30, moon's age respectively 9.13 and 10.07 days, may be observed.

1860, May 29, 9.50. Terminator just eastward of La Place. Clouds came over just as the observations were commenced, the streak across the floor was faintly visible, it was *nearly perpendicularly across*. The former greenish grey tint of the floor is now replaced by a darker tint

including a mixture of a blackish tinge, *i.e.*, instead of its being a greenish grey, it may be regarded as a light blackish grey. The shadow very near the western wall.

1860, May 30, 8.30, moon's age 10.07. Terminator just skirting the N.E. edge of the mountainous border of the Sinus Iridum. The high land in the neighbourhood of Bianchini casting a considerable shadow; the terminator also passes the eastern edge of Kepler and western edge of Gassendi.

North interior. The N.E. part well illuminated and the two dark oval marks well seen; the interior illumination commences on the N.W., about the locality of the narrow lateral valley noticed on the 28th.

North exterior. On the exterior close to this break or narrow valley is a mountainous ridge running in a northerly direction; it casts a short shadow. [This ridge is the western boundary of the crateriform depression north of Plato, it was observed by the Rev. R. S. Hutchings in March, see list for May.—W. R. B.] The ruggedness and descent from the summit at the present time is still very perceptible.

Eastern interior brilliant.

Eastern exterior. The lacuna of a triangular form very distinct, its interior floor dark; the rock ζ brilliant, casting a deep shadow. At the common base externally of the rock ζ and the lacuna is a small crater.

Floor rather darker than on the 29th, it is now approaching a "steel-grey." The light streak visible and more diagonal in its direction than on the 29th, the northern extremity abutting on the westernmost of the two dark oval markings on the interior of the north-east border.

Buckhurst Hill: May 17, 1878.

JUPITER.

1878. oh. Gr.	Angle of position of 24's axis.	Latitude of Earth Sun above 24's equator.		Equat. diameter.	Defect of illumination.
June 4	343°44	—0°56	—0°91	43.89	0.28
14	343°60	0°54	0°87	45.10	0.21
24	343°85	—0°53	—0°82	46.15	0.14

The assumed First Meridian passes the middle of the illuminated disk at the following times :

1878.	G. M. T.				
	h. m.		h. m.	h. m.	
June 1	1 39'1	June 6	10 39'1	June 11	19 39'1
	11 34'5		20 34'5	12	5 34'5
	21 29'9	7	6 29'9		15 29'9
2	7 25'3		16 25'3	13	1 25'3
	17 20'7	8	2 20'7		11 20'7
3	3 16'0		12 16'1		21 16'1
	13 11'4		22 11'5	14	7 11'4
	23 6'8	9	8 6'8		17 6'8
4	9 2'2		18 2'2	15	3 2'2
	18 57'6	10	3 57'6		12 57'6
5	4 53'0		13 53'0		22 52'9
	14 48'4		23 48'4	16	8 48'3
6	0 43'8	11	9 43'8		18 43'7

1878.	G. M. T.				
	h. m.		h. m.		h. m.
June 17	4 39'1	June 22	3 43'4	June 27	2 48'0
	14 34'4		13 38'9		12 43'4
18	0 29'8		23 34'3		22 38'7
	10 25'2	23	9 29'7	28	8 34'1
	20 20'6		19 25'0		18 29'4
19	6 15'9	24	5 20'4	29	4 24'8
	16 11'3		15 15'8		14 20'2
20	2 6'7	25	1 11'1	30	0 15'5
	12 2'1		11 6'5		10 10'9
	21 57'4		21 1'9		20 6'2
21	7 52'8	26	6 57'3	July 1	6 1'6
	17 48'2		16 52'6		
Assumed daily rate of rotation, 870° 60				A. M.	

The municipality of Reggio has resolved to honour the memory of the late distinguished astronomer, P. Angelo Secchi. 1. By putting a tablet in the house in which he was born. 2. By placing a bust in marble in the museum of the Storia Patria. 3. By giving the name of Angelo Secchi to a street of the city. 4. By inscribing his name amongst the other illustrious persons of the province in the vast galleries of the Royal Lyceum. 5. By enriching the library of the Commune with all the writings of Secchi.—*Reggio Emilia*, March 25th.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To June, 1878.

Hemming, Rev. B. F.
Lewis, H. K.
Squire, H.

To Dec., 1878.

Abbott, F.
Andrews, W.
Moore, W. E.

To Jan., 1879.

Daw, E.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will at once inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or penny postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for the Editor are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The *Astronomical Register* is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, payable in advance, by Penny postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, Angel Court, Throgmorton Street, E.C., not later than the 15th of the Month.

The Astronomical Register.

No. 187.

JULY.

1878.

ROYAL OBSERVATORY, GREENWICH.

The Annual Visitation of the Royal Observatory took place on Saturday, June 1, when the Astronomer-Royal presented his report to the Board of Visitors, and the rooms in which the various instruments are deposited, with the grounds, were thrown open to those members of scientific societies who had received cards of invitation from the President of the Royal Society, Chairman of the Board of Visitors. The company assembled was a select and brilliant one. Among those present we noticed the President and ex-President of the Royal Astronomical Society—Lord Lindsay and Dr. Huggins—also Dr. Lassell, Professors Pritchard and Adams, Messrs. Knott, Walker, Beck, and many other astronomical celebrities, including the Transit of Venus Observers—Capt. Tupman and Father Perry. While noting the presence of many astronomers who have comparatively but lately entered on their career, we missed the familiar faces of many whom we have often met during the last twenty or thirty years at similar gatherings. Death has done its work in thinning our numbers, but there is no lack of enthusiastic workers in our science to fall into their places, and carry on the noble and arduous work of practical astronomy.

On looking over the report of the Astronomer-Royal there is nothing that strikes us of a novel character; nor should we expect much novelty, since the routine work is of such important interest as to allow only of unavoidable interruptions as may arise from unfavourable weather, unexpected derangement of instruments, and such like. Observations of the ordinary character, have proceeded with the same regularity as in former years. Among the

VOL. XVI.

principal magnetical results we find that the mean westerly declination for 1877 is $18^{\circ} 57'$, the mean dip $67^{\circ} 40'$, and the most important of the meteorological, the mean temperature, $49^{\circ} 4$, the same as the average of the preceding 36 years.

ROYAL ASTRONOMICAL SOCIETY.

Session 1878—79.

The Last Meeting before the Long Vacation was held at the Society's Rooms at Burlington House, on Friday, June 14th, 1878.

Prof. Cayley, M.A., F.R.S., &c., *Vice-President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

M. Edward G. Deville, of Quebec.

Mr. John Frederick Main, B.A., D. Sc. of University College, Bristol,

were balloted for and duly elected Fellows of the Society.

Mr. Glaisher announced that fifty presents had been received since the last meeting, and the thanks of the Society were voted to the donors.

Prof. Adams, who was called upon by the President, said: The communication I have to make to the Society will be very short, and I am afraid it is not one that can be followed by all present in the room. It is a note *On a remarkable property in the analytical expression for the constant term in the reciprocal of the moon's radius vector*, or what is commonly called the constant term of the moon's horizontal parallax. This constant has been developed in the lunar theories of Plana, Pontécoulant, and the more recent one of Delaunay, in terms of certain constant quantities. These are the eccentricities of the orbits of the moon and earth, the inclination of the moon's orbit to the ecliptic, and the ratio of the mean motions of the sun and moon which is denoted by m . The constant part of the reciprocal of the radius vector contains terms depending on the powers and products of the squares of the eccentricity and inclination of the lunar orbit, each of these terms having a co-efficient which is a function of m and of the square of the eccentricity of the orbit of the earth. Plana found that the co-efficients B and C, of the terms of this expression, which respectively involve the square of the eccentricity and the square of the inclination, both vanished, as far as he carried the development, which was not very far, he only taking account of m^2 and m^3 . Pontécoulant carried the development of B and C as far as m^3 and found that the additional terms likewise vanished, but he did not take into account the portion of

these co-efficients depending on the square of the eccentricity of the earth's orbit.

The facts thus observed gave some probability to the presumption that the following terms in m would likewise be found to vanish. But Pontécoulant and Plana did not show at all why these different terms vanished, for the co-efficients were found in a number of different parts that did not vanish separately, but only when collected together into one sum. Therefore, of course, it was left doubtful how far this observed law might hold good, and it was evident that in order to find whether there is any such general law, one must adopt different methods from those of *Plana* or *Pontécoulant*. After reflecting on the subject for some time, I found a general proof of the property that these co-efficients B and C will identically vanish, no matter how far you carry the developments. That appeared to me a very remarkable property. After a considerable time, when I returned to the subject, I found a much simpler proof than had occurred to me at first, and it is that proof that I now beg to communicate to the Society. Much more recently I have considered the terms of the next higher order with respect to the eccentricity and inclination, and I have found two simple and remarkable relations between the co-efficients of these terms and the co-efficients of some of the leading terms of the analytical expressions for the mean motions of the lunar perigee and node.

Prof. Cayley: If I understand it rightly, the ordinary process gives these co-efficients in different parts, so that there is a verification in showing that these seem to exhibit these relations.

Prof. Adams: Quite so.

Mr. Neison: The communication of Prof. Adams has great interest for me. He has solved a problem which I have tried, but only with partial success. He shows that these co-efficients completely vanish, whereas I have only gone so far as to show that they vanish as far as m^5 .

Prof. Adams: Pontécoulant did the same.

Mr. Neison: These terms depend on the square and higher powers of the disturbing forces. I show that, as far as the second power of the disturbing force, these terms are multiplied by the co-efficient of the term in their argument, and as this co-efficient must be zero to give this constant portion of the parallax, these terms must vanish. These terms, B and C , therefore, as far as the second power of the disturbing force or to the order m^5 , must disappear. I have not yet extended it to the higher power of the disturbing force.

Prof. Adams: I do not take the powers of m . I prove that the co-efficients identically vanish without assuming any form of

development in m , which simplifies the matter very much. My investigation takes into account all powers of the disturbing force. I prove the property in question by showing that the function which gives rise to the terms in B and C is an exact differential of a certain function of the moon's co-ordinate, and therefore cannot have a constant part, such as these terms are. They must, therefore, identically vanish.

Captain Tupman read a paper *On the measurements of the Transit of Venus photographs*. He said that the photographs which had been measured were taken with the five photoheliographs made by Mr. Dallmeyer for the transit of Venus expeditions, on "patent plates," six inches square, the images of the sun being very nearly 3.9 inches in diameter. The dry process of Captain Abney was used throughout.

The measuring instrument, the determination of the errors of its glass millimetre scale, and the method of obtaining the optical distortion of the photoheliographs, had already been described in the Society's proceedings. It had been found by an elaborate investigation that the lines of equal distortion were sensibly circles concentric with the centre of the field. The actual correction for distortion for that zone of the field in which the points to be measured generally fell, was exhibited on the board, and was almost identical for all five instruments.

Before commencing the measures of a negative, the position of the line of centres was marked upon the film by a simple mechanical process. This operation had been performed independently by Mr. Burton and himself, with no sensible difference. He had paid no attention to the marks left by Mr. Burton on the plates, and found that his own coincided with them in direction. In placing the negative in the instrument, the circular carrier was turned about until the line of centres was truly parallel to the direction of the sliding motion of the microscopes.

When the negatives are placed under the microscope, with an amplification of only five or six diameters, the limbs of both planet and sun, even those which are pretty sharp to the unaided eye, become extremely indistinct, and the act of bisecting a limb with the wire or cross of the micrometer is mere guess work. The deposit of silver fades off gradually to nothing, and the denser the film the broader generally is the zone of fading off, and the more uncertain the measures. In many cases the difficulty is aggravated by ruggedness due to atmospheric disturbances, but the smooth and gradual fading off is the chief cause of uncertainty of measurements.

There is only *one* really sharp picture in the whole collection, including the Indian and Australian contingents, and that is

one of Captain Waterhouse's wet plates taken at Roorkee, with a Dallmeyer instrument precisely similar to the others.

It should be remarked that in these instruments the artist has attempted to unite the photographic and visual foci on the collodion film. No doubt some sharpness of the photographic image was thus sacrificed, but this has little or nothing to do with the unfortunate failure of the photography generally.

Each photograph had been measured six times by Mr. Burton, and six times by himself. He was not able to include in his series of measures all the photographs measured by Mr. Burton, for the reason that when some of them were viewed through the microscope he could see nothing to bisect, either from the extreme faintness of the film, or from its too gradual fading off.

Mr. Burton generally employed a cross of webs, but he had preferred a single very fine web, the breadth of which was eliminated in the mean by the mode of bisecting.

It had been suggested that the measuring instrument should possess the power of rotating the sun's image about a mechanical centre. This would be useful in some cases of rugged limbs, when the sun's image was not rendered elliptical by refraction, but in his opinion would make no material difference in the accuracy of measurement. The rotation could only be applied to the limbs of the sun, whereas, perhaps, the greatest difficulty had been at the limbs of the planet.

From the measures corrected for distortion were obtained the photographic diameters of the sun and of *Venus*, the former presumably enlarged, the latter diminished, by irradiation, in a sensibly equal degree. The sum of the measured diameters in millimetres was compared with the sum of the tabular diameters, subject to errors, for the scale value, and thus every photograph furnished its own scale.

The paper then gave the formulæ by which the measured distance of centres affected by errors of semi-diameters was compared with the tabular distance affected by errors of parallax, right ascension, and north polar distance. From each photograph was formed an equation involving all the unknown quantities, of which the errors of parallax and of semi-diameters were the more important.

The rigorous solution of the equations resulting from Mr. Burton's measures was—

$$\begin{aligned} \text{Mean Solar Parallax} &= 8.165 - .209 (dR + dr) \\ dR.A. &= +5.38 + .287 (dR + dr) \\ dN.P.D. &= -5.10 - .882 (dR + dr) \end{aligned}$$

The parallax deduced being absurdly small, altogether

inadmissible indeed, the Astronomer-Royal suggested that the quantity ($dR + dr$), or the sum of the corrections to the tabular semi-diameters, should be considered the only unknown, and that approximate values of the true solar parallax, and of the errors of R.A. and N.P.D. should be substituted in the equations. This was done, the mean solar parallax being taken at $8''.85$, $dR.A.$ as $+5''.81$, $dN.P.D.$ as $-5''.33$, which values resulted from the general solution of the whole of the contact equations, and the following values of $dR + dr$ were obtained:—

Station.	By Burton's Measures.	Number of Photographs Measured.	By Tupman's Measures.	Number of Photographs Measured.
Luxor ...	— $0''.96$	11	— $0''.13$	12
" ...	— $1''.18$	11	— $1''.61$	11
" ...	— $2''.21$	11		
Honolulu ..	— $1''.29$	11	— $0''.21$	12
" ...	— $1''.75$	11	— $0''.54$	12
" ..	— $0''.71$	10		
Rodriguez	+ $1''.19$	11	+ 2.44	9
" ...	+ $0''.33$	10	+ 2.49	6
" ...	+ $2''.10$	10	+ 2.74	8
" ...	+ $0''.14$	11	+ 1.27	10
" ...	+ $1''.46$	11	+ 2.31	9
Burnham, N.Z.	+ $0''.68$	13	{ + 1.89 + 1.38	3 10
Kerguelen	+ $1''.51$	8	—	0

The above is perhaps the best way to exhibit the nature of the discordances. They might also have been shown as apparent errors of the tabular distance of centres.

The discordances of any one station are too large to admit of the measures being employed with advantage for the determination of the solar parallax. They are due to inherent defects of the photographic images. The reason why at the two northern

stations the signs are all *minus*, while at the three southern they are all *plus*, is at present obscure, and he (Captain Tupman) was not prepared to offer any suggestion as to the cause.

The Astronomer-Royal: I will confine myself to one or two supplemental remarks on the paper which has just been read. In the first place, I should say we have taken very great pains with regard to all things that could possibly cause constant errors in the measurements of the photographs. The first thing was to assure ourselves of the accuracy of the engraved scale made for us by Mr. Simms, by means of which all the measures made from the photographs have been evaluated. We did not trust Mr. Simms's divisions, but we examined all the divisions most carefully. The scale contained 140 millimetres, or something like that. We took the middle 128 of them; divided by 2 we have 64, and by comparing these two halves we get the error of the middle of these 128 spaces. Then the 64 spaces were divided into 2 of 32, and by comparing each of these we got the error for every 32nd division. Each of these 32 spaces was divided into 2 of 16 and so on, and finally we got down to every division, and we are able to state with the utmost accuracy the error of every division of that scale, as referred to the first and last of the 128 divisions. Then another thing was to determine the amount of the distortion of the photographic field. For that purpose we used a scale of equal parts 16 feet long, carefully constructed by Mr. De La Rue for some purpose of his own. It is a beautiful piece of mechanical workmanship. But we did not trust to the accuracy of the divisions there; we tested them every one, relying only on the rigidity and good construction of the instrument. I may mention that this scale of equal parts was made by Mr. De La Rue some years ago, and it had been carefully preserved by him, and upon being left with me it was carefully preserved in the observatory, and it has turned up for a use more important than was originally intended: and thus it may be taken that if a thing is well made it will turn up for use repeatedly, over and over again. As I mentioned, we did not trust to Mr. De La Rue's construction as to the accuracy of the parts, but we examined them very severely. We then took photographs of the scale on different parts of the plates; and then by means of our millimetre scale examined whether the equal parts upon the scale were equal parts upon the photograph. If they were not equal parts that was taken as an effect of distortion. The distortion was not very great, and it came out uniformly, measure after measure, in such a way that we saw that our process was one of extreme accuracy. So far, sir, for the apparatus for measuring the photographs. With regard to

the results, I must say I am grievously disappointed. I had, when officially called upon, to express my opinion on the propriety of incurring the expense of the photographic work—although, as I then stated, I had some doubts upon it—yet I expressed myself favourable to it; and I hold myself responsible, in some degree, for having sanctioned so much expense and labour. I can only say I have been much disappointed. The images themselves are certainly very troublesome. The process pursued will be quite understood from what was shown on the board by Capt. Tupman.

The millimetre-measure of the distance between the centre of the sun and the centre of Venus upon the photograph is a certain and definite thing, inasmuch as it is the measure from the point which is half-way between the two limbs of the sun and the point which is the mean between two limbs of Venus. But it was necessary to get a scale for the valuation of that measure. The scale I adopted was the best I could get. I do not mean to say that it was infallible; it was to take the sum of the measures of the diameter of the sun and of the diameter of Venus; and the reason why I took that was that in taking the measure of the diameter of the sun we are affected by irradiation outwards, and in taking the diameter of Venus we are affected by an error of irradiation of the opposite kind, and we might suppose these two errors would annihilate each other. I am confident now that they do not annihilate each other, and that this is the source of the uncertainty or discordance of our results. I imagine that the discordance arises from two causes: one is, the very excessive brightness about Venus, which makes it look somewhat like a hat with a brim round it, and the excessively gradual degradation of light at the limb of the sun. It is a far more difficult practical problem than I could have imagined before the observations were made. All the photographs with the exception of one are very much affected by irradiation. The result of the measures made is that we get a parallax enormously discordant from that obtained from the other observations. From the eye observations, after the most careful discussion, Capt. Tupman fixes upon something like $8''.83$ —(I will not answer for the 100th part of a second)—or something like that. It is not the quantity we fixed upon at first, but after careful discussion we say $8''.83$. From the reduction of the photoheliographic measures, treated as well as we can, we get a parallax of $8''.2$, which is utterly irreconcilable with the other observations. I mean to say that such a discordance as six-tenths of a second would spoil every observation made with the eye, and would introduce the most glaring inconsistencies. Now, in this difficulty, I suggested to Capt. Tupman that he should, in order to check the results, assume that the results

obtained from the eye observations gave very correct values of the various elements, viz., the solar parallax, the relative error of the place of Venus in right ascension, and the error of Venus's polar distance—sum of the two radii of Venus and the sun. If we take these same values and apply them to the equations which represent the photographic results we get for the sum of the values of the radii of the sun and Venus the large discordances which were set out on the black board. This shows that at certain stations the diameter of the sun, or of Venus, or both, comes out too large, and at certain other stations too small. Why I cannot tell at all. I do not see at this moment any sufficient explanation of this, as, although I have no doubts whatever of the fact, that is the state in which we are left, and I am very sorry for it.

Prof. Cayley : Which plate was it that came out very sharp?

Capt. Tupman : It was a wet plate taken by Capt. Waterhouse in India.

Mr. Gill : It may interest the Society to find that though the Astronomer-Royal has correctly indicated one source of weakness of the photographic method, which is a tentative method—for exact photographic observation is nothing but experiment as yet—yet it must be so far satisfactory to find that, so far as reports reach us from other nations which have adopted photography for the transit of Venus observations, they have not been more successful than the British. For my own part, I attach a good deal of importance to the absolute determination of the scale value on the photograph, and great pains were taken at Mauritius to secure the absolute determination independent of diameters; but it seems from what Mr. Rutherford has quietly said in this country, that the American and French photographs have not been more successful than the English. Therefore, there is some subtle cause to be searched for which is reserved for the exact photographic observers of the future, and which certainly deserves attention if the planet Venus is to be observed by photography on a future occasion.

Mr. Brett : The Astronomer-Royal has spoken about the expense incurred in getting these photographs. It seems to me the money has been very well expended if it has ascertained for us the fact that the limb of Venus on the sun cannot be seen, which is what I should have expected.

Mr. Common : I would ask one question. In taking the photographs of the scale for determining the distortion, was the focus of the photoheliograph altered in any way?

Captain Tupman : Not materially. It was not the celestial focus, of course.

Mr. Common : Would not that introduce errors?

Capt. Tupman: I think not. Very little indeed, because the distortion is nearly all due to the secondary magnifier. The focussing is done by pushing the object-glass further away or drawing it back. The secondary magnifiers, reticule and collodian film, were not shifted.

Captain Tupman read extracts from a paper *On the Mean Solar Parallax derived from the telescopic observations of the Transit of Venus, 1874*, of which the following is an abstract:

The first part of the paper consisted of *verbatim* copies of those portions of the observer's original reports relating to the internal contacts which had not hitherto been published. They were given in order that astronomers might judge of the way in which the subject had been treated. He no longer believed in the possibility of maintaining the special phases of internal contact adopted in the Parliamentary Report, which were founded only on some of the British observations. He had confined himself in the present investigation to the phases of *contact complete* for both Ingress and Egress.

Treating the Ingress first, he had selected the times which seemed to him to correspond as nearly as possible to the phase of *Ingress complete*. The language of the observer was not always satisfactory as regards the definition of the phase. The parallax in reality depended upon the British observations; and unfortunately it was possible to take very different views of the observations in the Sandwich Islands. He accepted Mr. Stone's views of those observations.

The equations formed from the selected times were freed from the effects of errors of R.A. and N.P.D., which means simply that the geocentric position of the centre of Venus was taken into account. In the first solution every observation was given equal weight, with the result for mean solar parallax of $8''.863$. He, however, thought it proper to give double weight to the observations made with the large instruments under favourable atmospheric conditions, and to reject a few very wild ones, chiefly by amateurs, which were unimportant from the extreme smallness of their parallax factors. He then obtained a mean solar parallax of $8''.845$, but he considered that the observations were fairly satisfied by any value of the parallax between $8''.84$ and $8''.87$. The number of observations upon which this result practically depended was 14, four of which had received double weight. The residuals, being expressed in seconds of time as apparent errors of observation, were—

In 9 cases under 2s.
 „ 4 others under 5s.
 „ 4 others under 8s.
 „ 3 others under 12s.

while for the rejected observations they ranged from 17s. to 30s.

The Egress was then treated in a similar manner. There were in all 48 observations. Of these two were rejected because there was no guarantee for the local time, five because they appeared to be as much as 30s. in error. Of the remaining 41 he considered 14 entitled to double weight, and, thus weighted, the solution of the equations gave the mean solar parallax $8''.846$, showing an unexpected agreement within $.001$ with the ingress result. It was, however, to be remarked that there were four observations at Sydney, N. S. W., and one in India which had very great weight in producing a small parallax. If these observations were excluded the solution of the remainder, weighted as before, gave $8''.89$ for the solar parallax, but on this supposition the Sydney observations would be about 17s. too late. There was no reason whatever for rejecting the Sydney observations; on the contrary, he had given Mr. Russell's double weight. On the whole he thought the Egress observations were fairly satisfied by any value of the parallax between $8''.83$ and $8''.88$, although something between $8''.84$ and $8''.86$ was most probable.

The residuals for the egress observations, expressed as before, were—

In 20 cases under 5s.
 „ 14 others under 10s.
 „ 6 others under 13s.
 „ 1 other under 15s.

while the apparent errors of six of the rejected observations ranged from 16s. to 48s., and the remaining one was 90s. in error.

The general mean of Ingress and Egress, viz., $8''.8455$, corresponds to a mean distance of the sun of 92,400,000 miles.

Mr. Ranyard read a paper by Prof. C. A. Young, of Princetown, on his observations of the transit of Mercury. He had been unable to make out anything like a ring of light round the planet, or any bright spot upon the disk. Once or twice he thought that he had seen a tendency to a radial structure in the rice grains of the photosphere close to the limb of the planet, as if they had been lengthened out radially, but he was inclined to think that the phenomena was wholly subjective. The observations were made with a Merz polarizing eye-piece, and the planet appeared of a dark violet colour, much like the colour of sun-spots seen under similar circumstances.

Mr. Ranyard read a letter from Mr. Prince, of Tonbridge Wells, calling the attention of the Society to an observation that he had made of a spot upon the planet Mercury, when near to its greatest elongation, in June, 1867. An account of the observation was published in the *Astronomical Register* for August of that year.

It ran thus: "The atmosphere on June the 11th was so remarkably diaphanous, and the planet's disk so unusually distinct, that I observed for the first time a spot upon the surface a little south of the centre of the planet, with faint lines diverging from it to the north-east and south." This observation appeared so similar to many of the observations which had been made during the recent transit, that he thought it right to draw the attention of the Society to it.

Mr. Christie read a paper *On the existence of bright lines in the solar spectrum*. He said: This is a note I have drawn up on the question which Professor Draper has raised as to the existence of bright lines in the solar spectrum. I wish shortly to point out some of the difficulties that have occurred to me from an examination of the solar spectrum with a half-prism spectroscope. Without insisting too strongly upon them, I may state that with the spectroscope at Greenwich we could not discover any evidence of bright lines, though we found many fine dark lines not shown in Dr. Draper's photograph. I will draw on the board the spectrum in the neighbourhood of G, as seen at Greenwich, and endeavour to show what the point at issue is. Dr. Draper considers that two of these spaces correspond to two bright lines of the spectrum of oxygen. In Dr. Draper's photograph the dark lines and the spaces between appear of about equal breadth, but when you come to examine the sun with a high dispersive power, I find that the dark lines are perhaps one-tenth of the breadth of the bright spaces between, and that the spaces are perfectly uniform in tint, and there is not the slightest trace of fading off at the edges, as you would expect with a bright line of considerable breadth. It seems to me that that presents a great difficulty in accepting the conclusion that these spaces are really bright lines. Besides that, there are to be seen fine lines upon the bright spaces, so that we can hardly speak of these spaces as bright lines when they have two dark lines in the middle of each. If we suppose there are three bright lines together, we must adopt the hypothesis that these three bright lines are so close together that they almost touch, leaving a fine dark line between, and are sharply defined at the edge. That view presents far greater difficulty than to suppose that the spectrum is really a continuous background, with narrow dark lines of the width of the slit interrupting it. In comparing the whole solar spectrum, as far as I can see, there is no trace of any variation of tint whatever. I can only give this observation for what it is worth. It is an observation with one instrument, and the mere fact of not seeing a gradation of tint does not prove there is none. At the same time there seems considerable difficulty in accepting the view that the bright spaces are due to

bright lines, the main point being that the dark lines are narrow and that between them are placed spaces broad and perfectly sharp up to their edges, a condition which seems to me somewhat inconsistent with the usual character of bright lines in a metallic or gaseous spectrum.

The Astronomer-Royal: I understood that the angular separation used by you was far greater than that which Dr. Draper used.

Mr. Christie: I am hardly able to speak on that point, because I have been able only to compare these photographs, and it is hardly fair to compare a copy of a negative with an original, so that I should not like positively to say that our dispersion was greater than that used by him, though at the same time we can certainly see a great deal more, and with a good deal more sharpness than is to be seen in the copy of Professor Draper's photograph.

Mr. Ranyard said: This question as to the identity of the bright lines or interspaces with the bright lines of the oxygen spectrum is one of very great interest. It would be better if we spoke of all these matchings of lines as questions of probability. A coincidence can never be absolutely made out. If with any particular dispersion lines appear to coincide it may happen that with a larger dispersion the coincidence is found not to be exact. This, in fact, happened in the case of the 1474 line. At first it was thought to be coincident with one of the lines in the spectrum of iron, but Professor Yonng, on making use of a much larger dispersion, found that this was not absolutely so, but that there was a slight difference in the position of the two lines. What ought to be said with regard to Prof. Draper's announcement is—that with the dispersion he has made use of, some ten or eleven lines of the oxygen spectrum appear to match with ten or eleven bright lines or interspaces of the solar spectrum, and more than that, he showed, I think, that they matched in relative intensity. Mr. Christie has not shown, at present, that this is not the case, he has only examined the solar spectrum, and has made a map in which he finds broad bands which, to his eye, do not appear to fade off at the edges—but it is possible that similar bands or pairs of bands may be found in the oxygen spectrum.

With regard to the theoretical difficulty, that a mass of vapour above the photosphere must give rise to a dark line, we have the case of D_2 which is seen as a bright line in the chromosphere, and there is no dark Fraunhofer line corresponding to it; then there is also the case of the 1474 line, which is by far the brightest line of the coronal spectrum, and yet it is only found as a very faint dark line in the solar spectrum, which of itself would be enough to upset the theory of exchanges as ordinarily stated.

Then, again, we have the great gaseous masses forming the nebulae giving out bright lines, which shows that the outer and cooler parts of the nebulae do not absorb all the light of similar wave lengths emitted by the hot interior. I, therefore, do not see any theoretical difficulty in the supposition made by Professor Draper, that some of the elementary substances are, from something in their molecular constitution, incapable of absorbing all the light of similar wave lengths emitted from the interior.

Mr. Gill said: Mr. Schluster, who has paid some attention to the spectrum of oxygen, is here, and could no doubt say something on the subject.

Mr. Schluster: I have not much to say. I have not examined very much the supposed lines of oxygen in the violet part. There are some bright lines of oxygen near F, which I have carefully looked for in the solar atmosphere. The lines in the violet I do not know very well. I may say one thing—none of the lines of oxygen are sharp lines. Seen with a higher dispersion they may possibly be so; but the instrument to detect bright lines with is one of small dispersion. Yet there is no doubt you would have to expect a fading away of the bright lines from the middle towards the sides. As far as I, myself, am concerned, I should like very much that Professor Draper's discovery should be confirmed, but I confess at present I think the probability is the other way. I think the observations which I described in a letter to *Nature*, in December, show that oxygen gives at a lower temperature a different spectrum; and this spectrum, as far as I can judge, is reversed in the sun, so that the two would be present together. If Professor Draper's observations had been confirmed I should be confident that the lower temperature spectrum would be dark. With the lower temperature the spectrum is only of four lines, and I could only determine them with great inaccuracy, and they certainly seemed to be reversed in the sun. With regard to the theoretical question of bright lines in the sun, I do not think the question is so simple as Mr. Ranyard has just put it. In the nebulae you have not got that very bright background of continuous spectrum you have in the sun. From merely theoretical grounds it would be impossible to have bright lines in the solar spectrum. If you consider the sun as a body at rest with different layers of gases, hottest in the middle, and the temperature gradually decreasing as you get to the outside, no thickness of layer will produce a bright line, and the only assumption which can give you bright lines is the great disturbance of the body of the sun, which tends to throw upwards the gases from the middle of the sun towards the outside. If then you have the gas at a much higher

temperature in contact with gas at a lower temperature at the same level of the sun, then, I think, you can have bright lines, but not otherwise.

Mr. Christie: In my explanation I omitted to state one point, as to the coincidence of the lines. It seems to me that undue stress has been laid on this fact, seeing that none of the bright lines of oxygen coincide with dark lines of the sun. That is admitted, of course, because none of them have been identified with the dark lines, and, therefore, it follows that these bright lines of oxygen must coincide with the spaces between the dark lines, and if we can determine the coincidence within a limit of error of half the breadth of the space, which I should maintain is about the limit of error in Professor Draper's photograph, then the coincidence comes to nothing at all; because all we can say is that the bright lines of oxygen fall somewhere in that space, and we cannot say they coincide with the middle of the space, because the definition of the photograph and the dispersion is not sufficient to determine that point. It must be understood I am speaking entirely of the positive copies and not of the original negatives, because the definition may be much better on the latter, but I can only judge by the evidence laid before us, so that the fact of eleven lines, or of twelve, or any number proves nothing whatever, because we know there are no coincidences with the dark lines, and, therefore, there must be coincidences with the light spaces.

Now, with regard to definition I may mention that in our spectroscope at Greenwich, we can see 10 lines where Angstrom has only given three. He has given these three lines, but we see fine lines as well. Now, the test of definition is to see these fine lines in the spectrum. In that respect I think our definition has been sufficiently good, at any rate to show anything that can be shown in such a photograph as Professor Draper's. More lines are shown than, I believe, can be seen in Mr. Rutherford's photograph, which, however, is only a print.

Mr. Ranyard: Did you use a half-prism spectroscope?

Mr. Christie: Yes, a half-prism spectroscope. I may also mention that I have examined this spectrum, both with great dispersion and great purity of spectrum, that is to say by reversing the spectroscope. In getting great purity the lines are closer together and you see these fine lines sharply defined, so that if there were any bright lines they would be brought out with great vividness, and as a matter of fact we are able to see the lines in the chromosphere of the sun with very great distinctness, even when there is cloud or haze in the neighbourhood of the sun, so that one would expect to see some indication of bright lines if there were

any. There may be other ways of testing the point, but I do not see my way to any other mode of looking at it at present. At the same time I have only examined this part of the spectrum because this is the part where the coincidences seemed most marked.

Mr. Downing read a paper *On the proper motions of stars in the Greenwich seven-year Catalogue for 1864*. He stated that it was an attempt partly to supply for the first four hours of right ascension the deficiency as to the proper motions of the stars in Bradley's Catalogue. 176 stars had been examined and considerable discordances had been found with the proper motion given in the British Association Catalogue.

Mr. Christie read a paper giving the results recently obtained at Greenwich with regard to the motions of stars. The paper was a continuation of two former papers. In November last systematic observations were renewed with the 10 prism and half prism spectroscopes. On the whole the results were very accordant with both instruments. There was only one case of discordance of sign. The motions of 51 stars had been determined spectroscopically. In a former comparison with Mr. Huggins's results, the star β Leonis appeared to have an opposite motion than that found by Mr. Huggins, but now the motion agreed in sign with that determined by Mr. Huggins.

Mr. Ranyard: There are several papers on observations of the transit of Mercury, which as we have not much time I will not read. Dr. Konkoly saw no black drop, nor any arc of light round the planet, nor any white spot on its surface. Mr. Plummer saw, or thought he saw, the planet before it touched the sun's limb—his wife was sure she saw it.

Dr. Copeland gives an account of the observations made at Dunecht. No bright band was seen round the planet, and no spot of light was seen on its surface.

I have also a paper from Mr. Tebbutt on his observation of the great comet of 1861, which he observed at the time that the earth passed through its tail.

Mr. Neison read a paper *On the results of some investigations which he had recently undertaken of the theory of Mars*.

Mr. Green said that he had received a large number of drawings of the planet Mars, made during the recent opposition, and he proposed with the sanction of the Society to make a chart giving names to some further details which were not to be found in Mr. Proctor's maps. He would be sure not to put any Greenland into the chart, but he thought it would be proper to name some of the new markings after recent observers.

Mr. Knobel exhibited a chronograph tracing made by the clock at the Washington observatory. The remarkable accuracy of the

rate of the instrument was shown by the almost absolute straightness of the line of second marks upon the sheet which corresponded to a period of two hours.

The meeting adjourned at ten o'clock.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE STAR LALANDE 31266-7.
(Translation.)

In the June No. of the *Astronomical Register*, that I received this morning, I read a notice by Mr. J. E. Gore on the insertion of this star in the Atlas of Dien. This star is in fact found in the Catalogue of Lalande, but in consequence of an error of reduction, and is a false notification of Capella. Although I have already corrected various errors on the charts of Dien, this one was not perceived by me. I have corrected it for future editions. The star inscribed in the Catalogue of Lalande, under the Nos. 31266 and 31267 does not exist in fact.

Paris: June 1, 1878.

CAMILLE FLAMMARION.

COMES OF RIGEL.

Sir,—I can fully bear out the conclusion of the Rev. S. J. Johnson in regard to the magnitude of the companion of Rigel.

With a 9-inch silver speculum, I see it from 15m. to 30m. before sunset, whereas the companion of Polaris is not visible until sunset or a few minutes after. From its superior altitude in this latitude, and its greater distance from its primary, the companion of Polaris should, if they were almost equal, be seen the earlier of the two.

The following extracts from my observatory journal will show this:—

Comes of Rigel.—1878, March. 2d., 5h. 50m. G.M.T. Sunshine, easily seen and might have been caught earlier.

Comes of Polaris.—1878, May, 31d., 8h. 50m. Easily seen, and June, 5d., 8h. 45m., also easily seen.

Struve in "*Mensuræ Micrometricæ*" gives the magnitude of the companion of Rigel as 8, and that of Polaris as 9.

While on this subject it may be well to call attention to the companion of ϵ Draconis. I made an observation of it on March 4d., 20h. 30m. in bright sunshine, when it was quite easily seen. Mr. Webb calls it 9.5 mag., but says "variable? . . . easy to me and apparently underrated." Struve makes it in 1830.0 8.5, and in 1833.4 7.0 giving for the mean 7.6; it appears still to be about this magnitude.

I find Lord Lindsay's summary of the "*Mensuræ Micrometricæ*" most useful, for it often renders it unnecessary to turn to that unwieldy volume at all, or, if necessary, saves time by acting as an index to it, and finally it is not too large for reference in the observatory, for which the reduction epoch to 1875 eminently fits it.

If it could only be purchased through some publisher his Lordship would confer another benefit upon astronomers.

Your obedient servant,

Higber Bibington Hall, Birkenhead:
June 8, 1878.

R. C. JOHNSON.

Mr. Johnson will find that the Comes to Polaris is rated $9\frac{1}{2}$ mag. in "Celestial Objects," p. 301. Still there is certainly a much greater difference between the comes to Rigel and Polaris than stated by Smyth. The fact of the comes to Rigel being only some $9''$ from a first mag. star at a comparatively low altitude, whilst the comes to Polaris is $18''$ from a second mag. star at a great altitude, would be quite sufficient to establish its superiority, even if the two objects were seen equally well with the same aperture. I always rated it myself at about two mags. brighter than the comes to Polaris, but from the reasons above stated, there is not so much difference as test objects. I have seen both of them, on the same night, pretty steadily with $1\frac{1}{2}$ -inch stop on my 5-inch o.g. Is not Rigel supposed to be variable in colour? Sim and Fletcher give it pale yellow and yellow, respectively; Σ , yellowish white; Knott, white; but the author of "Celestial Objects" "sees a blue tinge, resembling Vega." To my eye it is a brilliant bluish white.

Leicester: June 1, 1878.

W. S. FRANKS.

STARS POSSIBLY VARIABLE.

The following notes extracted from a manuscript catalogue of 3,890 stars, may perhaps interest Mr. Gore.

		Date.	Mag.	Remarks.
(1)	66 Ceti.	Nov. 28, 1877.	6	Equal to 63.
(2)	π Ceti.	"	4	
	η Eridani.	"	3	
(3)	ρ^3 Eridani.	"	$5\frac{1}{2}$	
	ρ^3 "	"	5	Superior to ρ^2 .
(4)	τ^6 Eridani.	Nov. 30, 1877.	$4\frac{1}{2}$	Superior to τ^9 .
	τ^9 "	"	5	
(5)	63 Eridani.	Dec. 7, 1877.	6	
	64 "	"	$5\frac{1}{2}$	Superior to 63.
(8)	ρ Capricorni.	Sept. 12, 1877.	5	
	π "	"	$5\frac{1}{2}$	Inferior to ρ .
	β Corvi.	April 2, 1878.	$3\frac{1}{2}$	Sequence, $\gamma, \delta, \beta, \epsilon, \alpha$.

Leicester: June 1, 1878.

W. S. FRANKS.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN JULY, 1878.

By W. R. BIRT, F.R.A.S., P.S.S.

PITATUS, PLATO.—We class these formations together as they possess a feature in common. The Selenographical Society has issued an outline diagram of Pitatus and Hesiodus, and calls attention to three clefts or rills so disposed as to form three eboris to the mountain wall, encircling the interior plain; of these the longest is marked ξ , and numbered 291 in Schmidt's catalogue, it extends between the west and north points of the wall of Pitatus; the next in length is on the north-east, it is marked ϕ , and is 290 of Schmidt; the shortest is on the opposite or south-west side of Pitatus, it is 292 of Schmidt, and marked ψ . These clefts are,

near the border and convey the idea of having resulted from cracks just where the floor of the interior plain meets the foot of the inclosing mountain wall, there is a line of objects just inside the south-east wall, which seems to complete the series, viz., a line of craters, the interior ones confluent, the two end ones perfect.

The Rev R. S. Hutchings, of Salisbury, has discovered three clefts on the border of Plato, which with the well-known ravine exterior to Plato on the south-west, form a somewhat similar system to that of Pitatus. The Plato system appears to indicate that on the south-west and north-west fissures have been opened close to the border from nearly south by west to north, and that analagous convulsions have occurred on the eastern border. There is such a family likeness in this respect between the formations Pitatus and Plato, that it is not unlikely that other cases of the same kind may be found on searching for them. The somewhat large bays, clearly the remains of once perfect craters, situated on the borders of the Maria point to the operation of a very general cause as affecting both the formation of the large grey plains and the breaking down of the walls of craters in immediate contiguity with them at the time of their formation, which appears to be one of the best established instances of Selenology: in like manner were instances of clefts as presented in Pitatus and Plato found in other large walled plains, we may be on the eve of detecting another great selenological principle.

Buckhurst Hill: June 12, 1878.

THE PLANETS FOR JULY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	6 34 41	N.24 20	5".0	23 53.1
	9th	7 40 28	N.23 13	5".0	0 31.2
	17th	8 47 4	N.19 13	5".3	1 6.2
	25th	9 42 12	N.14 58	5".7	1 29.7
Venus ...	1st	4 1 28	N.19 35	14".3	21 20.3
	9th	4 40 8	N.20 23½	13".7	21 27.4
	17th	5 19 56	N.21 41	13".3	21 35.7
	25th	6 0 37	N.22 22	12".6	21 44.8
Jupiter ...	1st	20 30 17	S. 19 34½	44".3	13 50.4
	9th	20 26 37	S. 19 48½	44".7	13 15.3
	17th	20 22 35	S. 20 4	45".0	12 39.8
	25th	20 18 20	S. 20 19	45".2	12 4.1
Saturn ...	1st	0 13 49	S. 0 59	16".0	17 33.3
	9th	0 14 17	S. 0 58	16".2	17 2.3
	17th	0 14 22	S. 1 0½	16".4	16 30.9
	25th	0 14 3	S. 1 4½	16".6	15 59.2

Mercury rises shortly before the sun at the beginning of the month, after the 3rd he sets a few minutes after the sun, the interval increasing.

Venus may be observed from about an hour and a half after midnight throughout the month.

Jupiter rises about an hour and a quarter after sunset on the 1st, the interval decreasing.

Saturn rises half an hour before midnight at the beginning of the month, the interval increasing.

ASTRONOMICAL OCCURRENCES FOR JULY, 1878.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.		1st Ec. D.	h. m. s.	h. m.
Mon	1		Sidereal Time at Mean Noon, 6h. 37m. 37 ^s .39s.		13 13 38	Vega. 11 53'3
Tues	2	0	Conjunction of Moon and Mars 0° 33' N.	1st Sh. I.	10 24	11 49'3
				1st Tr. I.	10 57	
				1st Sh. I.	12 19	
				1st Sh. E.	12 44	
				1st Tr. E.	13 17	
Wed	3		Sun's Meridian Passage 3m. 55 ^s .18s. after Mean Noon	1st Oc. R.	10 32	11 45'4
Thur	4	1	Superior conjunction of Mercury and Sun Saturn's Ring: Major axis=40"·16 Minor axis= 3"·46	2nd Oc. R.	10 44	11 41'5
				3rd Sh. I.	11 13	
				3rd Tr. I.	13 12	
				3rd Sh. E.	14 46	
Fri	5					11 37'5
Sat	6	20 20	3 Moon's First Quarter			11 33'6
		8 24	Occultation of B.A.C. 4201 (6)			
		9 29	Reappearance of ditto			
Sun	7					11 29'7
Mon	8			1st Ec. D.	15 8 6	11 25'7
Tues	9			1st Sh. I.	12 18	11 21'8
				1st Tr. I.	12 41	
				1st Sh. E.	14 38	
				1st Sh. I.	14 56	
				1st Tr. E.	15 1	
Wed	10			2nd Tr. I.	15 41	11 17'9
				1st Ec. D.	9 36 47	
Thur	11			1st Oc. R.	12 16	11 13'9
				1st Sh. E.	9 6	
				2nd Ec. D.	9 21 11	
				1st Tr. E.	9 27	
				2nd Oc. R.	12 58	
Fri	12			3rd Sh. I.	15 12	11 10'0
Sat	13	22 54	3 Full Moon			11 6'1
		8 43	Occultation of B.A.C. 6562 (64)			
		9 58	Reappearance of ditto	4th Oc. R.	11 47	
Sun	14	15 4	Occultation of 4 Capricorni (6)			11 2'1
		15 48	Reappearance of ditto			
		21	Conjunction of Moon and Jupiter 0° 18' N.			

DATE.		Principal Occurrences.	Jupiter's Satellites.		Meridian Passage.
	h. m.			h. m. s.	h. m.
Mon	15	Near approach of ♄ Capricorni (4) Illuminated portion of disc of Venus = 0.790 Illuminated portion of disc of Mars = 0.988	3rd Oc. R.	9 46	10 58.2
Tues	16	Sidereal Time at Mean Noon 7h. 36m. 45.876.	1st Sh. I. 1st Tr. I.	14 12 14 24	10 54.7
Wed	17	Sun's Meridian Passage 5m. 51.6s. after Mean Noon	1st Ec. D. 1st Oc. R.	11 31 33 14 0	Jupiter. 12 39.8
Thur	18		1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E. 2nd Ec. D. 2nd Oc. D.	8 40 8 50 11 0 11 10 11 59 43 15 12	12 35.3
Fri	19		1st Oc. R.	8 26	12 30.9
Sat	20	Conjunction of Moon and Saturn	2nd Sh. E. 2nd Tr. E.	9 48 10 2	12 26.4
Sun	21		2th Sh. I. 2th Tr. I.	14 29 15 9	12 22.0
Mon	22	♄ Moon's Last Quarter Conjunction of Moon and Mercury 0° 19' N.	3rd Ec. D. 3rd Oc. R.	9 15 58 13 3	12 17.5
Tues	23		1st Sh. I. 1st Tr. I.	16 6 16 8	12 13.1
Wed	24	Jupiter at opposition to the Sun Saturn's Ring : Major axis = 41".55 Minor axis = 3".52	1st Oc. R.	15 44	12 8.6
Thur	25		1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E. 2nd Oc. D.	10 34 10 35 12 54 12 54 14 29	12 4.1
Fri	26	Conjunction of Moon and Venus 4° 35' S.	1st Ec. R.	10 10 30	11 59.7
Sat	27		2nd Tr. I. 2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	9 21 9 29 12 18 12 25	11 55.2
Sun	28	Conjunction of Venus and ♊ Geminorum 0° 7' N.			11 50.7
Mon	29	● New Moon Eclipse of the Sun, in- visible at Greenwich	3rd Oc. D.	12 41	11 46.3
Tues	30	Conjunction of Moon and Mars 2° 19' N.			11 41.8
Wed	31	Conjunction of Moon and Mercury 2° 17' N.	1st Oc. D.	15 8	11 37.3
Aug.			1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	12 17 12 29 14 37 14 49	11 32.9
Thur	1	Neptune at quadrature with the Sun			

JUPITER.

1878. Oh. Gr.	Angle of position of 24's axis.	Latitude of Earth Sun above 24's equator.		Equat. diameter.	Defect of illumination.
July 4	344°20	—0°53	—0°78	46°99	0°08
14	344°62	0°54	0°74	47°54	0°02
24	345°08	—0°56	—0°69	47°77	0°00

The assumed First Meridian passes the middle of the illuminated disk at the following times :

1878. G. M. T.

July	h. m.	July	h. m.	July	h. m.
1	6 16	12	0 09	22	8 49
	15 57°0		9 56°3		18 02
2	1 52°3		19 51°7	23	3 55°6
	11 47°7	13	5 47°0		13 51°0
	21 43°0		15 42°4		23 46°3
3	7 38°4	14	1 37°7	24	9 41°7
	17 33°8		11 33°1		19 37°0
4	3 29°1		21 28°4	25	5 32°4
	13 24°5	15	7 23°8		15 27°8
	23 19°9		17 19°1	26	1 23°1
5	9 15°2	16	3 14°5		11 18°5
	19 10°6		13 9°8		21 13°8
6	5 5°9		23 5°2	27	7 9°2
	15 1°3	17	9 0°6		17 4°6
7	0 56°7		18 55°9	28	2 59°9
	10 52°0	18	4 51°3		12 55°3
	20 47°4		14 46°6		22 50°7
8	6 42°7	19	0 42°0	29	8 46°1
	16 38°1		10 37°4		18 41°4
9	2 33°4		20 32°7	30	4 36°8
	12 28°8	20	6 28°1		14 32°2
	22 24°1		16 23°4	31	0 27°5
10	8 19°5	21	2 18°8		10 22°9
	18 14°9		12 14°2		20 18°3
11	4 10°2		22 9°5	Aug. 1	6 13°7
	14 5°6				16 9°1

A. MARTH.

THE INNER SATELLITES OF SATURN.

Major and minor semi-axes *a* *b* of the apparent orbits of the inner satellites :

Mimas.	Enceladus.		Tethys.		Dione.		Rhea. Eq. diam.	
<i>a</i> <i>b</i>	<i>a</i>	<i>b</i>	<i>a</i>	<i>b</i>	<i>a</i>	<i>b</i>	<i>a</i>	<i>b</i> of ball.
July 4.								
27°36	2°35	35°12	3°02	43°48	3°74	55°74	4°79	77°79
July 14.								
27°84	2°39	35°73	3°07	44°24	3°80	56°67	4°87	79°15
July 24.								
28°31	2°39	36°33	3°07	44°99	3°80	57°63	4°87	80°49

The earth being now at the south side of the plane of the orbits, the apparent motion of the satellites is in the opposite direction to that of late years, the satellites moving from the superior conjunctions in pos. angle, $183^{\circ}8$ (behind the ball) to their greatest eastern elongations in pos. $93^{\circ}8$, to their inferior conjunctions in pos. $3^{\circ}8$ (in front of the ball), etc., or in the direction of decreasing position angles.

Approximate Greenwich times of some of the conjunctions of the satellites with the ends of the ring, and also of some of their greatest elongations:

nf.	Sat. in conj. with following edge, north, moving towards the ball.
np.	" " preceding " " " away from "
sf.	" " following " south, " " "
sp.	" " preceding " " " towards "
e.	" at greatest eastern elongation.
w.	" " western "

1878. Gr. M. Time.

July	1	h.		
		13'9	Rhea.	sf.
		14'6	Dione.	nf.
		14'8	Tethys.	nf.
		15'7	Encel.	w.
	2	10'9	Dione.	w.
		12'4	Rhea.	e.
		13'1	Encel.	nf.
		13'5	Tethys.	sp.
	3	10'9	Rhea.	nf.
		12'1	Encel.	sf.
		12'1	Tethys.	nf.
	4	10'8	Tethys.	sp.
		14'4	Encel.	sp.
		16'2	Dione.	np.
	5	13'4	Encel.	np.
		16'6	Tethys.	np.
		17'1	Dione.	sp.
		17'1	Rhea.	sp.
	6	10'8	Encel.	e.
		13'5	Dione.	e.
		15'2	Tethys.	sf.
	7	13'9	Tethys.	np.
		14'4	Mimas.	e.
		14'8	Encel.	sf.
	8	10'8	Dione.	sp.
		12'1	Encel.	w.
		12'5	Tethys.	sf.
		13'0	Mimas.	e.
	9	11'2	Tethys.	np.
		11'6	Mimas.	e.
	10	13'5	Encel.	e.
		14'8	Rhea.	sf.
		16'0	Dione.	w.
		17'6	Tethys.	e.
	11	12'4	Dione.	sf.
		13'3	Rhea.	e.
		16'2	Tethys.	w.
	12	11'8	Rhea.	nf.
		13'3	Dione.	nf.

1878. Gr. M. Time.

July	12	h.		
		14'8	Encel.	w.
		14'9	Tethys.	e.
	13	12'2	Encel.	nf.
		13'5	Tethys.	w.
	14	12'2	Tethys.	e.
		16'1	Encel.	e.
	15	10'8	Tethys.	w.
		13'5	Encel.	sp.
		14'6	Mimas.	w.
		14'9	Dione.	np.
	16	9'5	Tethys.	e.
		12'5	Encel.	np.
		13'2	Mimas.	w.
		15'8	Dione.	sp.
	17	9'4	Rhea.	np.
		9'9	Encel.	e.
		11'8	Mimas.	w.
		12'2	Dione.	e.
		14'8	Encel.	nf.
		15'9	Tethys.	sp.
	18	13'8	Encel.	sf.
		14'6	Tethys.	nf.
	19	9'5	Dione.	sp.
		11'2	Encel.	w.
		13'2	Tethys.	sp.
		15'6	Rhea.	sf.
	20	11'9	Tethys.	nf.
		14'1	Rhea.	e.
	21	10'5	Tethys.	sp.
		12'5	Encel.	e.
		12'6	Rhea.	nf.
		14'7	Dione.	w.
	22	9'2	Tethys.	nf.
		9'9	Encel.	sp.
		11'0	Dione.	sf.
	23	12'0	Dione.	nf.
		13'9	Encel.	w.
		14'9	Mimas.	e.
		14'9	Tethys.	sf.

1878. Gr. M. Time.

	h.		
July 24	11'2	Encel.	nf.
	13'5	Mimas	e.
	13'6	Tethys.	np.
25	12'1	Mimas.	e.
	12'2	Tetbys.	sf.
	15'2	Encel.	e.
	17'2	Dione.	e.
26	10'2	Rhea.	np.
	10'7	Mimas.	e.
	10'9	Tethys.	np.
	12'6	Encel.	sp.
	13'6	Dione.	np.
27	9'5	Tethys.	sf.
	11'6	Encel.	np.
	14'5	Dione.	sp.
	16'5	Encel.	w.
	17'3	Tethys.	e.
28	10'9	Dione.	e.
	13'9	Encel.	nf.

1878. Gr. M. Time.

	h.		
July 28	16'0	Tethys.	w.
	16'4	Rhea.	sf.
29	12'9	Encel.	sf.
	14'6	Tethys.	e.
	14'9	Rhea.	e.
30	10'3	Encel.	w.
	13'3	Tethys.	w.
	13'4	Rhea.	nf.
	15'2	Encel.	sp.
	16'1	Dione.	sf.
31	11'9	Tethys.	e.
	14'2	Encel.	np.
	15'1	Mimas.	w.
	17'0	Dione.	nf.
Aug. 1	10'6	Tethys.	w.
	11'6	Encel.	e.
	13'4	Dione.	w.
	13'7	Mimas.	w.

On July 5 Iapetus reaches its greatest eastern elongation, and on July 25 its inferior conjunction, north of the planet.

A. MARTH.

Books Received.—Report of the Astronomer-Royal to the Board of Visitors of the Royal Observatory, Greenwich. 1878.—Star Gauging, Past and Present. By J. Norman Lockyer, F.R.S. London: Macmillan & Co. 1878.—The Electric Constitution of our Solar System. By Jacob Innes, Philadelphia. 1878.—Symon's British Rainfall, 1877. London: Edward Stanford. 1878.

ASTRO-OMICAL REGISTER—Subscriptions received by the Editor.

To Sept, 1878.

Birmingham, J.

To Dec., 1878.

Banks, W. L.
Chadwick, S. J.
Clegam, W. B.
Fleming, Rev. D.
Munson, W. H.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will at once inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or penny postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for the Editor are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The *Astronomical Register* is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, payable in advance, by Penny postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, Angel Court, Throgmorton Street, E.C., not later than the 15th of the Month.

The Astronomical Register.

No. 188.

AUGUST.

1878.

SUN-SPOTS AND THE WEATHER.

The Physical Observatory scheme is now a matter of history, by which it was pretended that the weather could be predicted from the sun-spots. Appeals from those interested in the matter were made to the public in the strongest language, through every channel to which the concoctors of the scheme had access, insisting upon the intimate connection between the sun and terrestrial meteorology; and urging that the one way to confer the power of vaticination upon meteorological science was to build and furnish a gigantic observatory at the cost of the nation.

It is instructive to note in the outset, that the claptrap uttered to this end, and with this intent, was, in the main, addressed to the unscientific public; an audience, as a rule, only too ready to listen with credulity to statements authoritatively made concerning subjects of which it is ignorant. Assuredly, though it failed to delude a single man of really high scientific position; and very notably was the idea scouted in the Royal Astronomical Society when an artful attempt was made to involve that body in a recommendation to the Government in its favour. In fact the disturbance which was subsequently made in that Society by certain members of the Physical Observatory party brought the whole matter so prominently into notice as (to some extent) to educate popular opinion in the matter; with the immediate result of scotching the observatory job for some time. It was, however, only scotched, and not really killed; for even up to this instant the persons interested in the matter do not seem to have lost hope of being able to fasten, like leeches, upon the national exchequer, and to help themselves to the

VOL. XVI.

public money under a quasi-scientific pretext. They have not even disdained for this purpose to trade upon that hideous calamity the Indian famine; and from time to time letters and paragraphs in inspired journals serve as straws to indicate the direction of that wind on which they so ardently hope to be wafted into pecuniary prosperity.

Our more immediate object here, however, is to discuss the question in its purely scientific aspect, and to examine the assertions which have been made with such (apparent) confidence, in the light of ascertained and irrefragable facts. A Mr. Meldrum, of the Mauritius, appears to be the chief apostle and prophet of the advocates of the sun-spot and weather theory, and his alleged discoveries shall therefore have precedence. The first paper of his which we can find any record of was read before the Meteorological Society of Mauritius, on October 10, 1872, its title being "On a supposed periodicity of rainfall." In it he deduces the fact (?) that sun-spots and rainfall wax and wane together. This was followed by a paper by Mr. J. Norman Lockyer, in *Nature*, for December 12, 1872, on "The meteorology of the future," which formed a reprint of a great deal of Mr. Meldrum's essay, and was written in support of his views. A fortnight afterwards a long article by Mr. G. J. Symons appeared in the same columns, in which our well-known English meteorologist, while admitting that "the evidence is conflicting," yet could not "help thinking that it is quite clear that the question must not rest where it is." Early in the succeeding year, when this number of *Nature* had presumably reached Mauritius, we find that a paper was read at the society there, asserting that "Mr. Symons's elaborate researches on British rainfall indicated, upon the whole, a similar periodicity," (*i.e.*, that years of maximum sun-spots were wet, and minimum sun-spots dry over England). We shall presently examine in detail into the truth of this. Later in the same year Mr. Meldrum brought forth some more (of what he considered) corroborative evidences of his views. So far the sun-spot party had all their own way. In the *Zeitschrift für Meteorologie*, for March 15, 1873, however, appeared a most able and exhaustive article by Dr. Jelinek, on this subject. Without entering here upon Dr. Jelinek's argument, it may suffice to give the results of a series of observations of rainfall, extending from 1833 to 1867, during which four periods of minima and three of maxima occurred. These observations were made at Guernsey, Greenwich, Orkney, Tarn, Toulouse, Bagès, Béost, Conaçon, Paris, Geneva, Great St. Bernard, Rome, Palermo, Calcutta, Algiers, Oran, Philadelphia, Fort Columbus, New Bedford, and Barbadoes; with the result that while fifty-one

records of annual rainfall seemed to favour the hypothesis, *fifty were dead against it*. It requires no very exhaustive knowledge of the theory of probabilities to enable any one to draw a very definite conclusion indeed from these figures; which need, therefore, detain us no longer. Nor is it necessary to advert to the various forms of *rechauffé* in which Mr. Meldrum (*toujours Meldrum*) was served up between the date of Dr. Jelinek's paper and 1876. We will, therefore, pass at once to the latter year, which was signalled by a now notorious article on "Indian famines and sun-spots," appearing in *The Times* for the 7th of June. In it it was definitely asserted that Dr. Hunter believed that he had discovered a method "for predicting beforehand the years in which a failure of the crops is likely to occur," the method in question being founded on the alleged coincidence which, since the year 1810, had existed between famine, drought, and sun-spot minima. The next manifesto of importance of the sun-spot party, appeared in the *XIXth Century* for November, 1877, and was signed by Messrs. Hunter and Lockyer, its distinguishing characteristic being a studious suppression of everything antagonistic to the interested views advanced, and the absolute misrepresentation of the results and opinions of Mr. Blanford, the official Indian meteorologist. It was further characterised by a demand for an Indian *meteorological* observatory, it being, we suppose, considered advisable to slightly disguise the character of the institution which so far had been so persistently begged for unsuccessfully under another name. The joint authors of this paper were either ignorant of—or curiously omitted to state—the fact that *one hundred and four* meteorological observatories were already at work in the Indian Peninsula! On the 4th of December, however, there appeared in *The Times* newspaper a most crushing rejoinder from Mr. Blanford, which, as a really scientific discussion of the whole question as applicable to India, we would gladly quote in its entirety, did not insufficient space preclude us from doing so. We may, however, give a short extract in which Mr. Blanford sums up the result of his investigations. "My conclusion," he says, "is, then, that we are as yet far from having discovered the laws of famine recurrence. As far as the evidence yet put forward can be said to point to any law of periodicity, it is this: that severe famines seem to tend to occur more frequently about the time of *minimum* sun-spots in Southern India, and about that of *maximum* sun-spots in Northern India, but the evidence is very imperfect, and requires thorough examination. It is unquestionable that a great deficiency of rainfall in one region, is, in many cases, attended by a great excess elsewhere. The rains which were withheld from

Madras at the close of 1876, were discharged over the Bay of Bengal, producing two severe cyclones; and while the North-West provinces, during the present summer, have suffered an almost entire loss of the crops of that season from continued drought, those of Pegu have been drowned and washed away by the extraordinary floods of the Irawaddy."

It is in reality in this extraordinary diversity of climate co-existing at various parts of the earth's surface, that the most conclusive answer to the suppliants for a Solar Observatory as a meteorological institution is to be found. It is worse than childish to suppose that the effect of the presence or absence of spots upon the sun's disk can be merely local. It must be either cosmical or non-existent. The elaborate researches of Mr. Baxendell, of Manchester, must be familiar to a large proportion of those who will read these lines, and it will not be forgotten how he found that, under precisely similar circumstances, a diametrically opposite condition of things, in regard to rainfall, obtained at Oxford and St. Petersburg. We have just seen, on the authority of Mr. Blanford that the climatic condition of various parts of the Indian Peninsula, differ simultaneously, *toto cælo*, and every attentive reader of the newspapers must have noticed from time to time concurrent accounts of extreme drought and of devastating floods in different portions of the surface of the globe. If we assume that it is the *same* sun which shines at Greenwich and at Galle, and if the activity of the solar photosphere really does act in increasing the amount of rain, then certainly a wet year in England should be a wet one in Ceylon, and *vice versa*.

But there is another cogent argument against the correlation of sun-spots and terrestrial weather. All the alleged remarkable coincidences between the spot periods and mundane meteorology have been based on the assumption that the sun-spot cycle is exactly an 11.11 year one; and the advocates of a "Physical Observatory" have been at great pains to show that rain, winds, tornadoes, auroras, magnetic phenomena and terrestrial temperature follow this period almost exactly. If this were true the idea of such advocates has been summarily disposed of by Dr. Wolf, of Zurich, who, in an exhaustive paper in Vol. XLIII. of the *Memoirs of the Royal Astronomical Society*, (p. 199), shows conclusively, from observations extending from 1610 to 1875, that the period in question holds only in the rudest way possible. For example, 1788 was a year of maximum spots, the next maximum period not occurring until 1804, *i.e.* after an interval of 16 years. Again, 1830 was a maximum year, as was 1837, 7 years only being thus the time between the maxima. Now, if the alleged co-existence between sun-spots and various

terrestrial phenomena really does exist it is obvious that such phenomena (including even those of the magnet), *should have passed through a 7 year cycle between 1830 and 1837*. Has it ever been pretended for a single instant that they did? We say nothing of the dictum of so very eminent an authority as the Astronomer-Royal, who, after a most elaborate examination of the Greenwich magnetic observations, denies the existence of any association whatever between the sun-spot period and terrestrial magnetism, but would merely ask what became of Messrs. Hunter and Lockyer's Indian Famine cycle during this particular interval?

Since however (as we have previously stated) it has been asserted definitely that "Mr. Symons's elaborate researches on British Rainfall indicated, upon the whole, a similar periodicity," we propose in conclusion to examine Mr. Symons's records year by year, between the years 1861 and 1873, in connection with Dr. Wolf's tables; inasmuch as this will enable our readers (and notably such as keep meteorological registers) to judge for themselves as to the truthfulness and *bona fides* of those who are asking for public money to enable them to predict the weather by the observation of sun-spots. For a description of the method by which Dr. Wolf derives his annual means, we must refer through the R. A. S. *Memoirs* (loc. cit.) themselves. Premising then, that against the year 1860 (one of maxima), stands in the table of "means" 94.8; we find against 1861 the number 77.7. Turning now to Mr. Symons's book, we learn that in 1861 in England, the total rainfall was 0.09, below the average of the preceding ten years; in Scotland 6.70 in. above it; and in Ireland 9.25 greater than the average. In 1862 Wolf's number is 61.0; and Symons returns the rain as being in excess in England 3.12 in.; in Scotland 7.70 in.; and in Ireland 6.60 in.; i.e., as the sun-spots decrease, the rainfall increases. In 1863 Wolf's mean falls to 45.4; and the amount of rain above the average so far falls with it that there is only an excess of 1.49 in. in England; 4.79 in. in Scotland; and 3.52 in. in Ireland. During 1864, the number of spots in the sun differed scarcely appreciably from that which was recorded in the previous year; 45.2 standing against the first-named year in Wolf's table. In England, however, the rainfall was 4.44 in. below the average 1850-59; in Scotland 3.08 above; and in Ireland 0.49 above it. Sun-spots continued to decrease in 1865, only 31.4 appearing as the mean for that year in question. Notwithstanding this rain was in excess in England 1.18 in., in Scotland 1.75 in., and in Ireland 7.32 in. 14.7 in. is the mean number which Wolf gives for 1866; how wet a year this was (19 per cent. above the

average in England, and the wettest since 1860) every British meteorologist who reads these lines will remember. In 1867, for the first time, we really do find something like a correspondence between the number of sun-spots and the quantity of rain. It was a year of minima, the number 8·8 being set against it as a mean by Wolf; and we find in referring to the British Rainfall, that taking the average from 1860-65, the rain in England was 0·12 in.; in Scotland 0·89 in.; and in Ireland 1·36 in. in defect. 1868, the year following a minimum (36·8 is Wolf's mean number), gave in 0·41 in. above the 1860-65 average in England; 5·01 in. in Scotland; and 2·00 in. in Ireland. In 1869 the number of sun-spots largely increased, 78·6 being Wolf's mean for that year; but the rainfall while 1·14 in. in excess in England, and 0·30 in excess in Ireland, was 2·19 in. in defect in Scotland: thus bringing the mean in the United Kingdom 0·75 in defect. 1870 was a year of maximum sun-spots, the large number 131·8, figuring against it in Wolf's table; but with an obstinacy which must have caused pain and grief to the advocates of a sun-spot observatory, the British weather absolutely declined to conform in the slightest degree to their theory, and 6·49 in. too little fell in England; 8·44 in. in Scotland; and 4·79 in. in Ireland; this, be it carefully observed, is a year in which more sun-spots were observed than had been visible since 1837. Nor did 1871 behave any better. In it the sun's disk was almost persistently covered with spots (113·8 is Wolf's number); but malgré this, the rain wholly refused to corroborate Mr. Meldrum's assertion concerning Mr. Symon's returns; and we find a deficiency in England of 2·13 in.; in Scotland of 2·37 in.; and in Ireland of 1·09 in. Sun-spots diminished to some extent in 1872, 99·7 standing opposite that date in Wolf's column of means. How tremendously wet a year it was nearly every one must remember in England, the rainfall was 13·73 in. above the average; in Scotland 11·42 in.; and in Ireland 11·63 in. Lastly in 1873, with a mean of 67·7, the rain was 2·51 in. in defect in England; 1·32 in. in Scotland; and 1·10 in Ireland.

Now these are dry, hard, irrefragable facts, which no amount of ingenious quibbling can alter. We have selected means accessible to all our readers of testing the truth of one (and that the chief) of the allegations on which the clamour for a Physical Observatory has been founded. With what result must be instantly apparent to every one who has followed us so far. *Ex uno disce omnes.* Need we pursue the subject further?

REVIEWS.

Lohrmann's Lunar Map. Mondkarte in 25 Sectionen von W. G. Lohrmann, mit beschreibendem Text von Dr. J. F. Julius Schmidt. Verlag von J. A. Barth in Leipsig. Price 50s.

A desideratum long awaited by astronomers has been at length supplied by the publication of Lohrmann's complete map of the moon. The first part of his *Topographie, &c.*, which contained only four sections of the twenty-five composing the map, was published so long ago as 1824, and no more of this great work was issued; but in 1838 a small map, comprising all the sections on a reduced scale, was lithographed by Werner at Dresden.

Lohrmann's lunar survey commenced in Dresden in 1821, and was finished in 1836. In 1840 he died, and Mädler, in the mean time, having stepped into the field, the work of the other fell for a period into neglect; but, fortunately, his manuscripts were in the hands of a true lover of science—Herr W. A. Barth, of Leipsig—who, at his private cost, undertook their publication according to the original plan of the author. In this he was assisted by Herr Opelt of Dresden, who revised the calculations and the tables, and superintended the engraving. The work, however, proceeding but slowly, Herr Barth, in February 1851, applied for the assistance of Dr. Schmidt, of the Athens observatory. Though Dr. Schmidt was at the time fully engaged with his own lunar map, he willingly responded to the call, and the work was quickly progressing when Herr Barth died in December of the same year. It was now fortunate that his son, Dr. A. A. Barth, determined to carry out the wishes of his father; and, after a lapse of two years, Dr. Schmidt resumed his labours at the map. Dr. Schmidt got his appointment as Director of the observatory at Athens in 1858; and the work then chiefly fell on Opelt, who was assisted by his son, a First-Lieutenant in the Saxon army; and the task was continued by the latter, after the death of his father in 1863.

Another death now occurred which might have been fatal to the final success of the undertaking, namely, the death of Dr. A. A. Barth, were it not that he was succeeded by a man of equal zeal in the cause of science, Herr J. A. Barth, who was determined to accomplish the design of his predecessors, and Lohrmann's 25 Sections have, accordingly, at length appeared with a descriptive letterpress written by Dr. Schmidt, who also completed the final revision and correction of the tables.

The map of 3 French feet, measures the $3\frac{1}{2}$ millionth of the moon's diameter. The new text, by Schmidt, is on a considerably abridged plan compared with Lohrmann's. The parts connected with general astronomy are omitted; so also are the biographical notes relating to lunar nomenclature, and the method of calculating and fixing positions. On the other hand, Lohrmann's account of his mode of delineation is retained, and a list of positions by Opelt is added. Lohrmann's minute descriptions are also modified, as Dr. Schmidt considers that the various minor objects are sufficiently well outlined on the map, and require no special reference in the text.

In the preface to the published part of his work Lohrmann explained that his design was to represent the lunar mountains and shadings as closely as possible; and, following the methods of measuring and representation as prescribed by science, he adopted the orthographic projection of the lunar surface seen at mean libration.

As might be naturally expected, in a work of engraving taken in hand

at intervals during a space of fifty years, a very unequal style of performance in the different plates was the result, and the several tones of shading, which even in the Lohrmann's originals were not always satisfactory, appeared in very different proportions in different parts of the map.

To correct these completely would involve too much labour and expense, but some of the most important features have been retouched by Dr. Schmidt, who also has struck out several names that impaired the clearness of delineation.

To the largely increasing class of astronomers, professional and amateur, who make the lunar surface their study, and who have depended hitherto chiefly on Beer and Mädler's map, the publication of Lohrmann's complete sections must be highly appreciated. Schmidt's own wonderful map of 6 feet diameter will soon appear, and then we shall have the opportunity of testing our own observations by comparison with the lunar features as represented in no less than three great works. Evidences of continued volcanic activity in the moon may now be sought with increased facility, while at the same time, the almost cotemporary maps of Lohrmann, and B. and M. may serve to show that many apparent differences are due to the mode, or accidents of sketching rather than to change.

Too much praise cannot be given to Herr J. A. Barth for his liberal expenditure in carrying out the excellent design of his predecessors; and it is to be hoped that the undoubted value of the work will insure a demand that will amply reimburse a large outlay.

The astronomical world must surely recognize the good fortune that placed the task of editing in hands like those of Dr. Schmidt, whose disinterested conduct in suspending his labours at his own map for the purpose of publishing the work of another, and giving it precedence, is quite in keeping with the estimable character and scientific devotedness of the chief of the Athens observatory.

J. BIRMINGHAM.

The Unseen Universe, or Physical Speculations on a Future State. By B. Stewart and P. G. Tait. Sixth edition (revised and enlarged). London: Macmillan & Co. 1876. Price 6s.

(Continued from page 102.)

Ch. IV. is on *Matter and Ether*. After an account of the various hypotheses concerning matter, the authors well remark, "The reader who has followed us so far, must now see that our notions of the nature of matter are, at best, but hazy. We know, it is true, a great many of its properties very exactly, but as to *what it is* we know no more than Democritus or Lucretius did, though as to what it may be or may not be we are, perhaps, considerably better prepared with an opinion than they could possibly be." (pp. 141, 142.) And about Ether: "Regard the ether as we please, there can be no doubt that its properties are of a much higher order in the arcana of nature than those of tangible matter; and as even the high-priests of science still find the latter far beyond their comprehension, it would not become us to speculate further. It is sufficient for our purpose to know from what the ether certainly does that it is capable of vastly more than anyone has yet ventured to guess." (pp. 153, 154.) It was before concluded that in far remote ages the visible universe, having lost all its available energy, would become a "huge useless inert mass." This chapter closes with the speculation of its possible disappearance altogether.

Ch. V. on *Development* includes *Chemical, Globe, and Life* development. Under the second head we have the hypothesis of Kant and Laplace, and the tendency to aggregation of mass; and it is remarked that the very fact that the large masses of the visible universe are of finite size, is sufficient to assure us that the process cannot have been going on for ever; or, in other words, that the visible universe must have had its origin in time, and we may conclude that if the visible universe be finite in mass the process will ultimately come to an end. Under the last head the views of Darwin, Wallace, and Huxley are expounded; and in conclusion we read, "It may, perhaps, eventually be possible by means of an hypothesis of evolution, to account for the great variety of living forms, on the supposition of a single primordial germ to begin with; but the difficulty still remains how to account for this germ. It is against all true scientific experience that life can appear without the intervention of a living antecedent. How then are we to explain the production of the primordial germ?"

Ch. VI. is on *Speculations as to the possibility of superior intelligences in the visible universe.*

Having considered the decadence of the visible universe, and seen that it affords no basis for possible immortality; in the last chapter, VIII., our attention is directed to the apparently wasteful character of the arrangements of the visible universe. All but a very small portion of the sun's heat goes day by day into what we call empty space, and it is only this very small remainder which can be made use of by the various planets for purposes of their own. . . . That all but a petty fraction of this vast store of high-class energy should be doing nothing but travelling onwards in space at the rate of 188,000 miles per second is hardly conceivable, especially when the result of it is the inevitable destruction of the visible universe, unless we imagine this to be infinite, and so capable of endless degradation. If, however, we continue to dwell upon this astounding phenomenon, we begin to perceive that we are not entitled to assert that this luminous energy does nothing but continue to travel outwards. . . . We have already maintained (Art. 151.), that we cannot regard the ether as a perfect fluid, and that we must look to the unseen portion of the universe as that out of which the seen has been developed. We must, therefore, regard this unseen as possessing an extremely complicated structure. Now, it is not easy to suppose that in such a substance all vibratory motion should pass outwards without in the smallest degree becoming absorbed or changing its type. We are prepared, doubtless, to expect a great difference between the unseen and visible matter in this respect, but can hardly imagine that the unseen is absolutely free from the capacity of altering the type of the energy which passes through it. Such a hypothesis appears to us to violate the principle of continuity. . . . Again, we are compelled to imagine (Art. 152.) that what we see has originated in the unseen, and we must resort to the unseen not only for the origin of the molecules of the visible universe, but also for an explanation of the forces which animate these molecules (Art. 150.). So that we are compelled to conclude that every motion of the visible universe is caused by the unseen, and that its energy is ultimately carried again into the unseen. . . . In fine, it appears to us less likely that by far the larger portion of the high-class energy of the present universe is travelling outwards into space with an immense velocity, than that it is being gradually transferred into an invisible order of things. . . . If now we turn to thought, we find (Art. 59.) that, inasmuch as it affects the substance of the present visible universe, it produces a material organ of memory.

But the motions which accompany thought must also affect the invisible order of things, while the forces which cause these motions are likewise derived from the same region, and thus it follows "*Thought conceived to affect the matter of another universe simultaneously with this may explain a future state.*" This idea, however, requires further development and explanation. Let us therefore begin by supposing that we possess a frame, or the rudiments of a frame, connecting us with the invisible universe, which we may call the spiritual body. Now, each thought we think is accompanied by certain molecular motions and displacements in the brain, and parts of these, let us allow, are in some way stored up in that organ, so as to produce what may be termed our material or physical memory. Other parts of these motions are, however, communicated to the spiritual or invisible body, and are there stored up, forming a memory which may be made use of when that body is free to exercise its functions. Again, one of the arguments (Art. 84.) which proves the existence of the invisible universe, demands that it shall be full of energy when the present universe is defunct. We can, therefore, very well imagine that after death, when the spiritual body is free to exercise its functions, it may be replete with energy, and have eminently the power of action in the present, retaining also, as we have shown above, a hold upon the past, inasmuch as the memory of past events has been stored up in it, and thus preserving the two essential requisites (Art. 61) of a continuous intelligent existence. (pp. 197—200.)

Objections of a religious, theological, scientific, and quasi-scientific nature are next replied to. It appears to us that the authors have misapprehended the meaning of the two Scriptural passages referred to (p. 203). Many will, probably, agree with them when they say (p. 203) "We are logically constrained to admit the existence of some frame or organ which is not of this earth, and which survives dissolution—if we regard the principle of continuity and the doctrine of a future state as both true." It may indeed be questioned whether pure immateriality can be predicated of any created mind. Nor can any objection be made to the term spiritual body provided it be kept distinct from the resurrection body for which that term is used in Scripture. Perhaps ethereal body might be a preferable designation. As to the objection (p. 204), a future life for all animals has been held to be very probable by many persons. Proceeding to Miracles and the Resurrection of Christ, it is shown that development from the unseen has produced the visible universe, and that the atoms of the latter have all the marks of being manufactured articles. Thus the argument is in favour of the production of the visible universe by means of an intelligent agency residing in the invisible universe (pp. 214, 215), of the nature of which we are profoundly ignorant as far as science is concerned. Turning, therefore, to the Christian records, they are found to support the conclusions which have been arrived at concerning the development of the universe. And the same agreement is found to exist between the deductions of science on the development of life, and the testimony of Scripture. Life proceeds from life; dead matter cannot produce a living organism; and the conclusion from this law of Biogenesis is that life comes from the unseen universe (pp. 222—230).

After considering other hypotheses, the authors thus review the position they have reached: "By taking the universe as we find it and regarding each occurrence in it, without exception, as something upon which it was meant that we should exercise our intellects, we are led at once to the principle of continuity, which asserts that we shall never be carried from the conditioned to the unconditioned, but only from one order of the fully

conditioned to another. Two great laws come before us—the one of which is the conservation of mass and of energy, that is to say, conservation of the objective element of the universe; while the other is the law of biogenesis, in virtue of which the appearance of a living being in the universe denotes the existence of an antecedent possessing life. We are led from these two great laws, as well as from the principle of continuity, to regard as at least the most probable solution, that there is an intelligent Agent operating in the universe, one of whose functions it is to develop energy, and also that there is an intelligent Agent, one of whose functions it is to develop life. Perhaps we ought rather to say that, if we are not driven to this conclusion, it appears to be at least that which most simply and naturally satisfies the principle of continuity. But this conclusion hardly differs from the Christian doctrine, or to speak properly, the conclusion, so far as it goes, appears to agree with the Christian doctrine." (pp. 244, 245.)

It is next shown that miracles, viewed as the result of a peculiar action of the invisible upon the visible universe, do not involve absolute breaks of continuity: "When we dig up an ant-hill, we perform an operation which, to the inhabitants of the hill, is mysteriously perplexing, far transcending their experience, but we know very well that the whole affair happens without any breach of continuity of the laws of the universe." . . . "It is of course assumed that the visible and invisible are and have been constantly in a state of intimate mutual relation." (p. 246.) This is followed by some just remarks on the peculiar communication with the unseen in the case of Christ. And equally formidable (apparent) breaks are brought before us by science. "There is, to begin with, that formidable phenomenon, the production in time of the visible universe. Secondly, there is a break hardly less formidable, the original production of life; and there is, thirdly, that break recognised by Wallace and his school of natural history, which seems to have occurred at the first production of man." "From our point of view," say the authors, these "will be found to contain avenues leading up to the invisible universe." . . . "There may be a close and vital union between the two universes, in those regions into which investigation cannot penetrate, and who shall say that the laws of these regions do not admit of the objective efficacy of prayer? There may be an action of the invisible world upon the individual mind, and there is no reason why there should not also be an action upon the invisible universe, by means of those processes of delicacy which, as we have already seen, obtain in that quarter (Art. 184). Neither the one action nor the other would be detected by science. . . . (pp. 247—249.) Man should live for the unseen, and carry into it something which may not be wholly unacceptable. But in order to enable him to do this, the unseen must also work upon him, and its influence must pervade his spiritual nature. Thus a life for the unseen *through* the unseen is to be regarded as the only perfect life. The Christian records are again consulted for information otherwise unattainable on this head, and in them we learn about angelic intelligences, the doctrine of a particular providence, &c., and this part closes with the following pertinent remarks: "If we cannot succeed in avoiding contradictions in our notions of elementary geometry, can we expect that the ultimate purposes of existence shall present themselves to us with perfect clearness? I can see nothing to forbid the notion that in a higher state of intelligence much that is now obscure may become clear. We perpetually find ourselves in the position of finite minds attempting infinite problems, and can we be sure that where we see contradiction an infinite intelligence might not discern perfect logical harmony?" (p. 254.)

"The outcome of all these speculations . . . has conducted us to the conclusion that the doctrine of the Trinity, or something analogous to it, forms, as it were, the avenue through which the universe itself leads us up to the conception of the infinite and eternal One." Nevertheless, the authors think that the argument may be very much detached from all conceptions of the Divine essence. "We have merely to take the universe as it is, and, adopting the principle of continuity, insist upon an endless chain of events, all fully conditioned, however far we go either backwards or forwards. This process leads at once to the conception of an invisible universe, and to see that immortality is possible without a break of continuity. We have, however, no physical proof in favour of it, unless we allow that Christ rose from the dead. But it will be admitted that, if Christ rose from the dead, a future state becomes more than possible, it becomes probable. . . . Again, the production of the invisible universe in time leads us, by the principle of continuity, to the conception of a fully conditioned intelligent universe, existing prior to the production of the visible. And furthermore, we are induced by our argument (Art. 218) to regard the production of the visible universe as the work of an intelligent agency residing in the invisible. If, then, such an agency could produce the visible universe, it could certainly accomplish the resurrection of Christ, without any break of continuity, so far as the whole universe is concerned." (pp. 255, 256.)

Some considerations follow on the conditions of the future life (pp. 258—260), and a glance is cast on the darker side of the future (with a quotation from Plato on the markings of the soul); on the Christian Gehenna, the mediæval idea of hell, and the personality of Satan (pp. 260—268), and at the conclusion of the work the authors say, "Our readers are now in a position to perceive the result of questioning science in this manner, and of abandoning ourselves without mistrust or hesitation to the guidance of legitimate principles. It is that science so developed, instead of appearing antagonistic to the claims of Christianity, is in reality its most efficient supporter, and that the burden of showing how the early Christians got hold of a constitution of the unseen universe altogether different from any other cosmogony, but similar to that which modern science proclaims, is transferred to the shoulders of the opponents of Christianity. . . . The truth is that science and religion neither are nor can be two fields of knowledge with no possible communication between them. Such a hypothesis is simply absurd. There is, undoubtedly, an avenue leading from the one to the other, but this avenue is through the unseen universe, and unfortunately it has been walled up and ticketed with, '*No road this way*,' professedly alike in the name of science at the one end, and in the name of religion at the other."

We have left ourselves without space for criticism, but we hope those of our readers, by whom the book is yet unread, will in many cases be induced by the extracts we have given to procure it for themselves. At all events they may be glad to have such a general idea of it as these portions, selected with some pains, may afford them. For ourselves, we have found very much more in it that we can agree with, than we question, or dissent from. We think the general line of argument is sound and convincing, and—considering the nicety of some of the points discussed, and the difficulty at times of finding faultless phraseology in which to clothe certain ideas—that these physical speculations on a future state have, upon the whole, been expressed in language that is judicious and happy. The book is probably destined to have a yet wider circulation than it has already obtained, and Messrs. Stewart and Tait,

we doubt not, will not fail to improve it where they see occasion. It is pleasant to find it pervaded by a dispassionate tone, and to recognise the modesty and teachableness that are not less commendable in the philosopher than the divine, but which sometimes might be more apparent in both than is the case.

1. *The Discovery of the Force which in the beginning put all the heavens and the earth in motion.* 2. *The Four great Eras in Modern Astronomy.* 3. *The Meteors and their long-enduring tails.* By Jacob Ennis, A.M., Principal and Proprietor of the Scientific and Classical Institute, Philadelphia, PA. Cambridge. Press of John Wilson & Son, 1871, 1872.

These three papers were read at meetings of the American Association for the Advancement of Science, and extracted from their Proceedings at Indianapolis, August, 1870, and 1871.

"The four eras stand related to one another in this way:—Copernicus discovered the true motions of the stars in their orbits; Laplace discovered how these motions of the stars might have been produced; Newton discovered the *force* which now holds the stars in their orbits. The fourth era has discovered the *force* which, in the beginning, put all the stars in motion in their orbits." Laplace, however, it should be remembered, never claimed for his theory the value of a "discovery." He found that calculating the probability of the phenomena of the planetary system there was more than two hundred thousand milliards (a milliard is a thousand millions) to one against those phenomena being the result of chance, and, therefore, concluded that a primitive cause directed them; and he speaks of his nebular theory as a *hypothesis* which seemed to him to have a great likelihood, but which he offered with the diffidence which should be inspired by all that is not the result of observation or of calculation. Professor Ennis, indeed, has not overlooked this passage, as we fancied he had at first, but it is not quite to his taste, for he says Laplace "closes with these rather melancholy words." But though it appears to us that the author would have done better to have substituted the word *theory* for *discovery*, and would have more commended his works had they been pervaded by the "diffidence" of the great French geometer, we are of opinion that there is much in them worthy of attention, and this we hope they will receive from astronomers in this country. Mr. Ennis's work "*On the Origin of the Stars*," was noticed in the *Astronomical Register* of February last; and the two first of the above papers appear to be outlines of that larger work. The paper on the Eras concludes as follows:—"It is an instructive fact that none of these great astronomical eras have appeared such to the generation in which they were made. The theory of Copernicus, announced in 1543, lay almost dormant until the researches of Kepler and Galileo, in the beginning of the next century. No printer would undertake the publication of Newton's '*Principia*,' not even the learned Royal Philosophical Society of London. It was brought out in 1687, by the private expense of Halley, very much to his own inconvenience. At that period the Vortical theory of Descartes was believed and taught in the Universities, and a grand point was thought to be gained many years afterwards when Newton's doctrines were covertly introduced as foot-notes in the text-book of Descartes! Still, a few influential men in London appreciated his work, and he was rewarded with the Directorship of the Mint. The nebular theory of Laplace was published in 1796, and we all know how it was neglected by two generations. Now, at

length, no epitome of astronomy is thought complete without a brief explanation of that theory. These facts are here mentioned to awaken astronomers to the announcement of the fourth era in modern astronomy. The discovery of the force which, in the beginning, put all the heavens and the earth in motion, is not inferior to either of the three great announcements of "Copernicus, of Newton, and of Laplace."

As the Newtonian theory involves the Copernican theory, and as the nebular theory is founded on the truth both of the theory of Newton and that of Copernicus, so this fourth era includes the ideas of the former three, and goes far beyond them. It announces the first movements of creation! It begins with the action of gravity on the diffusion of nebulous matter slowly contracting. It shows how gravity separated that diffusion into clouds; then how gravity rounded and rotated those clouds, and disposed them in sidereal, solar, and planetary systems. This fourth era reveals the great instrument in the hand of Omnipotence, by which the constitution of the material universe was formed! By the constitution of the universe, I mean its systems of revolving and rotating globes, and the distribution of those systems through infinity of space." The paper on *The Discovery of the Force, &c.*, concludes thus:—"My own discoveries have at length placed the nebular theory on a plain mathematical foundation. The chief of them are these:—1st. That gravity must necessarily and unavoidably produce rotation as well as rotundity in all nebulae. 2nd. That gravity may increase that rotation so rapidly as to afford a centrifugal equal to the centripetal force. 3rd. That in nearly all nebulae forming solar and planetary systems, rotation must begin on the surface, and hence must be retarded by the unrotating or slowly rotating interior. This retardation accounts for a vast number of most important stellar phenomena. Nothing in my calculations opposes the doctrine of equal areas swept by the *radius vector* in equal times. If the nebula forming our solar system, and extending at first half way to the nearest fixed star, had originally a rotation velocity of only the one-third of a mile per hour, then its *radius vector* would have swept over an area equal to that of Mercury, whose orbital velocity is 110,000 miles per hour. In speaking above of Laplace, I should have added that he not only said nothing about the precise velocity of nebular rotation, but he gave no cause whatever for the origin of that rotation."

The paper on meteors is a very interesting one. Mr. Ennis gives nine instances of the trails of meteors enduring for about one hour. He undertakes to prove, 1st. That some meteors undergo a process of burning or oxidation while passing through the air, and that the trails are the smoke and ashes of such burning. 2nd. That some meteors are composed of various simple chemical elements unoxidised, and which are, therefore, capable of burning in the air; and, 3rd, to show the order and process of creation by which such meteors were originally formed, and left in an unoxidised condition. "We have evidence," he says, "of three very different kinds of meteor trails never before discriminated. First, meteors of phosphorus, whose trails have a fiery glow, and which disappear instantly throughout. Second, meteors composed of some easily combustible simple elements, such as potassium, sodium, calcium, magnesium, and the like. They burn and their ashy trails seem white and cloudlike; and, after remaining visible from a few moments to an hour or more, they disappear by a slow dispersion in the atmosphere. Thirdly, meteors composed of oxidised materials, stony meteors, and also metallic iron meteors. Their light and heat spring from the compression and the latent heat of the air, not from mere friction as is generally supposed. They may be so intensely heated on the surfaces as to be ignited and

vitrified, and their glowing exteriors may leave behind a short fiery trail instantaneous only in duration." Mr. Ennis thinks that the truth of the combustion theory is evidenced by the circumstance of the meteors of the November stream becoming incandescent at a greater height than those of the August stream; and, therefore, the former are composed of very different simple elements from those of the latter, and so burn at a higher altitude.—p. 13. "The simple elements in meteors are twenty in number, besides oxygen. If sixteen of these exist unoxidised, contrary to all terrestrial experience, why may not the other four, potassium, sodium, lithium, and strontium? There is, indeed, no reason why we may not see a meteor of potassium and sodium unoxidised, as well as a meteor of iron and nickel unoxidised. Existing alone in such a condition, the potassium meteors cannot possibly come down to the surface of the earth. They must necessarily burn brightly and beautifully on entering our atmosphere; and if they be large enough and near enough, they must leave a white visible trail stretching along their paths, and waving slowly on the breeze."—p. 16. Under the third head the author's theory is, that the meteors which reach the surface of the earth, stony meteorites, and metallic meteorites, come from the region of the solar system, within the ring of the asteroids. Secondly, that the unoxidised meteors, which burn and leave their phosphorescent and their ashy trails in the air, come mostly, probably altogether, from the more distant regions of the solar system, beyond the ring of the asteroids. Thirdly, that the meteors with visible trails were left unoxidised because, at the period of their creation, the simple element oxygen had not yet been formed, and, therefore, that the era of the creation of oxygen, as well as of many other elements, was not before the creation of the ring of the asteroids."—p. 17. "Enough has now been said to show that there are two classes of meteors, corresponding to the two classes of planets,—the outer and the inner, the distant and the near; the one yielding heavy, stony, and metallic bodies, often wholly oxidised; the other is unoxidised, and they burn spontaneously in the air, leaving phosphorescent and ashy trails. The ring of the asteroids seems to be the dividing line between them; and this fact is significant respecting both the original formation of the ring, and also the cause of the very wide difference between the outer and the inner classes of planets and meteors."—p. 21. In regard to the absence of about forty of our terrestrial elements from the composition of meteorites, Mr. Ennis remarks, "All these numerous facts lead us to believe that the simple chemical elements—mere modifications of some original form of matter—have been formed successively in the process of nebular condensation. At first, in the outer regions of the solar system, they were very few and very light. Then more and more were brought into being, of a denser constitution, both in the general solar and in the several planetary nebulae. The means by which they were created—the physical forces—and the proofs of the process, I have dwelt on at length in 'The Origin of the Stars.' Amongst other things explained by this view is the vast number of additional elements in the sun. 'The fixed lines in the solar spectrum show that hundreds of elements have been formed in the nebular condensation of the sun since the parting of our earth as a nebulous ring.' It also accounts for the vast variety of the simple elements in the fixed stars, as seen in their dark spectral lines. Although all have been formed from the same general diffusion of matter; still, in condensing, each one has varied from the others in forming its elements, following the rule that no two objects in creation are alike."—p. 25.

In the latter part of the paper Mr. Ennis describes the various

explanations that have been given of the ignition of meteors. "While the theory of combustion, or the burning of the meteors in the air, accounts for the light and heat of the great majority of the shooting stars, it does not account for all. Some meteors are composed of oxidised stones, and others of solid masses of iron, and these plainly cannot burn. Previous to the burning theory developed in this paper, there have been three other theories for the ignition of meteors. The one of these, which is now very generally adopted, ascribes the heat to the friction of the meteor against the air." "The theory of latent heat seems to be scarcely known. It is founded on the fact that the air contains what is called latent heat. The higher and more rare, the greater is the capacity of the air for latent heat. Therefore, in the extreme upper regions of the atmosphere, the greatest abundance of the latent heat is lodged. It is revived and rendered sensible by the compression and condensation of the air in front of the meteor as it flies along. Hence meteors ignite at very great heights; and when they come down to the denser strata of the atmosphere, where the latent heat is less abundant, the light of the meteor invariably goes out. This is proof of the most decisive nature in favour of the latent heat theory, and against the friction theory. In the lower and denser atmospheric strata, the friction must be the strongest; and, according to the friction theory, the heat and light should be the most brilliant. But instead of this, these strata are the regions where the meteors suddenly and invariably cease to shine. Not one has ever been seen to come incandescent to the ground."—p. 28. Mr. Ennis proceeds to adduce facts and reasonings in support of the above; and remarks (p. 31), that "the theory of the luminosity of meteors, not by friction, but by the latent heat of the air, was begun by Poisson,* applied and illustrated by Sir John Herschel,† and more fully developed by Mr. B. V. Marsh, merchant, and like Benjamin Franklin, an amateur scientist of Philadelphia." The demonstration and table by Marsh follow, with Mr. Ennis's comments, in which he claims to have rectified certain defects and omissions in Marsh's demonstration, with reference also to the atmospheric theory of Quetelet, in his work "*Sur la Physique du Globe*," 1861.

Astronomical Observations taken to the end of 1877, at the private observatory of Joseph Gurney Barclay, Esq., F.R.A.S., Leyton, Essex. Vol. IV. London: Williams & Norgate, 14, Henrietta Street. 1878. pp. 123.

Contents: Introduction—Double-star observations—Notes to double-star observations—Observations of comets—Eclipses, occultations of stars by the moon, and phenomena of Jupiter's satellites—Discovery of the companion of Procyon. Mr. Barclay having received many applications for copies of the earlier volumes of the Leyton Observations, after the edition had been exhausted, has in this volume included epochs of double-star measures from the commencement. The observatory and instruments are described in the Introduction. The telescope, mounted equatorially in the German fashion, is a powerful refractor of 10-in. aperture, and 12-ft. focal length, by T. Cooke & Sons, of York, with eye pieces magnifying from 50 to 1600 times, and furnished with wire, double-image and ring micrometers. The finder is 3-ft. focal length and 3-in. aperture, which shows stars to the ninth magnitude. The transit-circle carries a telescope of 4-in. aperture, furnished with two circles, 3-ft. in diameter,

* "*Ann. de Chim.*," xxlii., 341.

† "*Outlines of Astronomy*," p. 617; and "*Edinburgh Review*," Jan. 1848, p. 195.

divided to every five minutes, and read by four microscopes. The field is illuminated by gas. It must be a great satisfaction to a gentleman of means, and endowed with an astronomical taste, to be able to avail himself of a powerful instrument whenever he is inclined, and to know that continuous and valuable observations are being made under his auspices, and periodically given to the astronomical world. Private observatories, in which work is systematically carried on have been very useful. We wish there were more of them like Mr. Barclay's. Giving due honour to him who deserves so well of the science, he is also to be congratulated on having the services of an astronomer like Mr. Talmage, by whom the whole of these observations and the necessary reductions have been made. We have had the pleasure of more than one visit to this observatory, which is delightfully situated in the beautiful grounds surrounding the residence of Mr. Barclay at Leyton—to our idea a sort of terrestrial, and in some sense a celestial, paradise. The present volume, especially the double-star observations, and the valuable notes to them, cannot fail of being exceedingly acceptable to those occupied with this department.

The Electric Constitution of our Solar System. By Jacob Ennis. From the proceedings of the Academy of Natural Sciences, Philadelphia, for 1878. Philadelphia. 1878. pp. 19.

The following extracts show the drift of this pamphlet:—"The zodiacal light, the aurora borealis, the corona of the sun, and the tails of comets, are all different forms of the same thing. They are electrical hushes, precisely the same as the electric hushes which in the night are seen to fly off from a highly charged electric machine. On the electric machine the electric fluid is developed by friction. On our great globe, on the sun, and on the comets, the electric fluid is developed by evaporation." . . . "No one has said that the zodiacal light is electric, and that this and the auroral streamers, always like comets, point away from the sun. No one has regarded all these phenomena as emanating from a common cause; the solar heat causing evaporation. No one has supposed that the far distant solar corona, by mere electric repulsion, drives off the tails of comets, the auroral streamers, and the zodiacal light. A highly charged electric machine affects a delicate electrometer far off on the opposite side of a wide room; but this wonder, until now, was not fruitful in originating our theory. Now, first, all those distant facts have been brought together, and they are seen to form a harmonious system."—pp. 3 and 19.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE TRANSIT OF MERCURY, 6-7th MAY, 1878.

Sir,—Having observed the recent transit of Mercury with a fine Cooke equatorial of 4.25-in. aperture, and about 5-ft. focal length, kindly placed

at my disposal by H. C. Russell, Esq., F.R.A.S., Government astronomer I venture to append a few notes on the phenomenon, as seen in this distant part of the globe, thinking it might interest some of your readers. Here, in Sydney, we only saw the latter portion of the transit, the sun rising on Tuesday, 7th May, at 6h. 40m., by which time considerably more than half of the planet's course across the solar disk had taken place. It was not until some time after sunrise that the sun by clearing the clouds and vapours that hung round the horizon allowed us to see what was going on. There was the planet, small, round, and as definition improved, intensely black, hastening towards egress. The mottling on the sun's disk came out clearly with the powers used, 100 and 180, and as there was a total absence of spots, no comparison between them and the planet could be made. I could not make out any ring or halo surrounding the planet, nor could I see the white central spot so frequently observed at former transits (*vide* Huggins and Browning observations in 1868) although close scrutiny was made for it. About six seconds before apparent internal contact at egress, a shadowy film suddenly formed between the limbs of sun and planet, and connecting the limbs of these two bodies. Apparent internal contact was noted at 8h. 50m. 37s., local mean time; external contact at 8h. 52m. 11.7s., the planet completely disappearing as it glided off the sun's face—whereas I vividly remember seeing Venns in complete outline long before ingress was over at the transit of 1874, using the same telescope that I had on this occasion. Definition, owing to low altitude of the sun, and unsteadiness of the air, prevented any accurate micrometrical measurements.

Mr. Russell, with the large equatorial by Schröder, of 11½-in. aperture, made the times of contact somewhat different to mine, viz., internal contact at 8h. 49m. 43.1s., external, 8h. 52m. 53.1s., his increased aperture giving better definition. The morning was not very favourable for successful observing, a good deal of haze and clouds impairing telescopic vision.

I am, Sir, yours faithfully,

Sydney, New South Wales:

W. J. MACDONNELL

May 8, 1878.

P.S.—I may mention that I have been a subscriber for many years to your periodical, and always look forward to its arrival here with lively interest. Long may the *A. R.* prosper.

M. RAMBOSSON AND THE GREAT COMET OF 1861.

Sir,—An English translation by C. B. Pitman of Rambosson's *Astronomy* appeared in the year 1875, and a new edition (1878) of this translation has just reached the Colony. In the chapter on comets will be found the following passage:—

"There have been many remarkable comets of recent years. Upon the 8th of January, 1862, M. Winnecke, of Poulkova, observed a telescopic comet from three to four minutes in diameter, and it subsequently transpired that the same comet had been discovered by Mr. Tattle in America, nine days beforehand. Another comet, visible to the naked eye for those who had good sight, was seen on the 2nd of July, at about 10 p.m., by M. Schmidt, director of Baron Sina's observatory at Athens. This comet appeared quite suddenly, travelling in the direction of the north pole, and reminding one in the manner of its arrival of the great comet of 1861 as it first appeared in Europe. The latter, seven weeks before becoming visible upon our continent, had been clearly seen in the

southern hemisphere, by myself, amongst others, in the Isle de la Réunion. It was visible about 7.30 p.m. in the north-east, just about the sea-line. It gave a faint light, not greater than that of a star of the third magnitude, but on the other hand its crest, pointing eastward, extended nearly 18 degrees in length, even as seen with the naked eye."

If the above statement were correct it would appear that the author virtually claims for *himself and others* the honour of having observed the comet as soon as myself, but I should be glad to know where the original announcement of so surprising a discovery is to be found. Is it to be found in the *Astronomical* or other periodicals of May, 1861, or simply in Rambosson's *Astronomy* of recent date? My own discovery was communicated to the Sydney observatory on May 21st, and to the *Sydney Morning Herald* of May 25th of the same year. My first approximation to the elements of the comet's orbits appeared in the *Herald* of June 15th, together with a prediction of the close approach of the earth to the comet's tail for June 29th, and of the probable visibility of the comet itself in full daylight. This communication was followed by a more complete prediction by me in the *Sydney Empire* of June 22nd. My determination of the elements was twelve days the earliest published in this Colony, and I think it extremely improbable that anyone in the southern hemisphere can justly lay claim either to an earlier discovery or to an earlier calculation of the approximate orbit. I very much regret that I was not in correspondence at the time with the scientific journals of Europe. A communication of mine, very much garbled, was published in the *Illustrated London News* of December 28th, 1861.

M. Rambosson's incidental statements respecting the comet are very erroneous. Astronomers need not be informed on this point, because they have the means of detecting the inaccuracies, but it is well that the public, for whose instruction his popular work is written, should know the facts of the case. On the 13th May, 1861, or seven weeks before its appearance in the northern hemisphere, the comet was an excessively faint object, without either a nucleus or a tail, and it did not present a tail or become generally visible till about the beginning of June, or four or five weeks before it was seen in Europe. It was visible in the evenings in the south-west, and not in the north-east as stated by Rambosson.

JOHN TEBBUTT, F.R.A.S.

Private Observatory, Windsor, N. S. Wales :

April 23rd, 1878.

PARTIAL ECLIPSE OF THE MOON,

August 12, 1878, visible at Greenwich.

	h.	m.	G.M.T.
First contact with the penumbra, Aug. 12	9	22.5	
First contact with the shadow...	10	42.6	
Middle of the eclipse ...	12	8.2	
Last contact with the shadow ...	13	33.8	
Last contact with the penumbra ...	14	53.9	
Magnitude of the eclipse (moon's diameter = 1) 0.590.			

The first contact of the shadow occurs at 109° from the northernmost point of the moon's limb towards the east.

The last contact at 157° towards the west. In each case for *direct* vision.

TEMPEL'S COMET OF 1873.

Sir,—The following information may be of interest to some of your readers.

ELEMENTS.

Epoch of Perihelion passage = 1878, August 27.

Longitude of Perihelion = 306° 7'

Longitude of Ascending Node = 120° 59'

Inclination of Orbit ... = 12° 45'

Heliocentric Motion:— +

Calculator:—Schulhof.

EPHEMERIS.

For Paris Mean Noon.

1878.		R. A.			Decl.	
		h.	m.	s.	°	'
August 2	...	15	53	45	...	11 57
4	...	15	57	9	...	12 51
6	...	16	0	45	...	13 46
8	...	16	4	34	...	14 41
10	...	16	8	35	...	15 36
12	...	16	12	49	...	16 31
14	...	16	17	16	...	17 25
16	...	16	21	55	...	18 18
18	...	16	26	47	...	19 11
20	...	16	31	51	...	20 3
22	...	16	37	6	...	20 55
24	...	16	42	33	...	21 46
26	...	16	48	13	...	22 35
28	...	16	54	4	...	23 23
30	...	17	0	7	...	24 10

I am, Sir, Your obedient servant,

G. F. CHAMBERS.

Northfield Observatory,
Eastbourne: July 15, 1878.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN
AUGUST, 1878.

By W. R. BIRT, F.R.A.S., P.S.S.

DR. KLEIN'S NEW CRATER NEAR HYGINUS. Observations of this object have been received up to May, 1878. Should any of our readers have observed it in July, June, or earlier, I shall esteem it a favour if they will kindly forward notes of their observations to me. The following features should claim especial attention: *First Hyginus*. The position of this well-known object relative to the new crater. *Second*, the remarkable spiral mountain N. of Hyginus. The Rev. R. S. Hutchings, of Salisbury, has sent me a drawing, reproduced in the *English Mechanic*, No. 693, July 5, p. 497, in which only the eastern part of this spiral mountain is shown, and between it and Klein's new crater he gives a large elliptical depression surrounded by wide ridges. In all the maps hitherto published, not a trace of this depression with its surrounding ridges is to be found, it is therefore a matter of great importance that the observation of Mr. Hutchings should be confirmed. The state of Dr. Klein's new crater is

quite detached from the elliptical depression of Hutchings, who is very particular in describing the surrounding objects. Klein's new crater is situated S.W. of it, this he marks 6, a craterlet he places N.W. of it, marked 8, and one rather larger marked 10, is situated on the ridge N. of it. He says, "6, 8, 10, are three craters standing in the positions marked, round an oval depression 12, surrounded by wide ridges." It appears that during May, on the 9th, 10th and 11th, Messrs. Neilson, Sadler, and Knott observed an "elliptical dark grey marking," a "dark oval shading," and an "oblong oval shading," respectively. It is uncertain if these were really Klein's crater or Hutchings' depression, and it is necessary for rendering the literature or history of the district of Hyginus intelligible, that it should be clearly ascertained if the small round dark spot seen on April 10, 1878, moon's age, 7 6 days, by Dr. Klein as a greyish diffused mark with a darker nucleus, be a distinct object from Mr. Hutchings' much larger elliptical depressions surrounded by wide ridges. The evenings of the 5th, 6th, and 7th of August will be most suitable for observing the locality of these objects.

9, Grove Crescent, Stratford, E.:
July 20, 1878.

THE PLANETS FOR AUGUST.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	10 21 55	N. 10 26½	6".2	1 41.8
	9th	10 58 28	N. 5 24½	6".8	1 46.8
	17th	11 24 55	N. 1 5½	7".8	1 41.7
	25th	11 37 57	S. 1 43	9".0	1 23.2
Venus ...	1st	6 36 41	N. 22 26½	12".2	21 53.2
	9th	7 18 1	N. 21 54	11".8	22 3.0
	17th	7 59 7	N. 20 41	11".6	22 12.5
	25th	8 39 38	N. 18 50	11".2	22 21.5
Jupiter ...	1st	20 14 37	S. 20 32	45".0	11 32.9
	9th	20 10 31	S. 20 45½	44".8	10 57.4
	17th	20 6 47	S. 20 57½	44".4	10 22.2
	25th	20 3 35	S. 21 7½	43".8	9 47.5
Saturn ...	1st	0 13 28	S. 1 10½	16".8	15 31.1
	9th	0 12 27	S. 1 19	17".0	14 58.6
	17th	0 11 5	S. 1 30	17".2	14 25.8
	25th	0 9 26	S. 1 42½	17".4	13 52.7

Mercury may be observed for nearly an hour after sunset on the 1st, the interval decreasing.

Venus rises an hour and three quarters after midnight, at the beginning of the month, the interval increasing.

Mars sets about half an hour after sunset on the 1st, the interval decreasing.

Jupiter may be seen till three hours after midnight, at the beginning of the month, the interval decreasing to an hour and a half on the last day.

Saturn rises about an hour and a half after sunset on the 1st day, the interval decreasing.

ASTRONOMICAL OCCURRENCES FOR AUGUST, 1878.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Thur	1	19	Neptune at quadrature with the Sun Sidereal Time at Mean Noon, 8h. 39m. 50 ^s .67s.	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	12 17 12 29 14 37 14 49	Jupiter. — 11 32 ⁹
Fri	2		Sun's Meridian Passage 6m. 1 ^s .80s. after Mean Noon	1st Oc. D. 1st Ec. R.	9 34 12 5 21	11 28 ⁴
Sat	3			1st Tr. E. 1st Sh. E. 2nd Tr. I. 2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	9 3 9 18 11 37 12 7 14 34 15 3	11 24 ⁰
Sun	4					11 19 ⁵
Mon	5	1 19	☾ Moon's First Quarter	2nd Ec. R.	9 17 10	11 15 ¹
Tues	6	7 34 8 10	Occultation of 4 Scorpii (6) Reappearance of ditto			11 10 ⁷
Wed	7			4th Sh. I. 4th Tr. E. 4th Sh. E.	8 36 10 6 13 11	11 6 ²
Thur	8	7 49 9 1	Occultation of B.A.C. 6063 (6½) Reappearance of ditto	1st Tr. I. 1st Sh. I.	14 1 14 23	11 1 ⁸
Fri	9	8 40	Near approach of σ Sagittarii (2½)	3rd Tr. E. 3rd Sh. E. 3rd Oc. D. 1st Ec. R.	9 14 10 48 11 19 14 0 17	10 57 ⁴
Sat	10	21	Conjunction of Moon and Jupiter 0° 34' N.	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E. 2nd Tr. I. 2nd Sh. I.	8 27 8 52 10 47 11 12 13 54 14 44	10 52 ⁹
Sun	11			1st Ec. R.	8 29 6	10 48 ⁵
Mon	12	12 16	☾ Full Moon Eclipse of the Moon visible at Greenwich	2nd Oc. D. 2nd Ec. R.	8 4 11 52 39	10 44 ¹
Tues	13		Saturn's Ring: Major axis=42".77 Minor axis= 3".35			10 39 ⁷
Wed	14					10 35 ³
Thur	15	13 16 14 17 16 32	Occultation of B.A.C. 8276 (6½) Reappearance of ditto Near approach of 22 Piscium (6) Illuminated portion of disc of Venus=0 ^s .873 Illuminated portion of disc of Mars=0 ^s .997	4th Oc. D.	11 42	10 30 ⁹

DATE.		Principal Occurrences.	Jupiter's Satellites.	Meridian Passage.
		h. m.	h. m. s.	h. m.
Fri	16	6 Conjunction of Moon and Saturn $7^{\circ} 12' S.$	3rd Tr. I. 8 58 3rd Sh. I. 11 12 3rd Tr. E. 12 34 1st Oc. D. 13 4	Jupiter. — 10 26.5
Sat	17	Sidereal Time at Mean Noon 9h. 42m. 55.57s.	1st Tr. I. 10 12 1st Sh. I. 10 47 1st Tr. E. 12 32 1st Sh. E. 13 7	10 22.2
Sun	18	Sun's Meridian Passage 3m. 40.10s. after Mean Noon	1st Oc. R. 7 30	10 17.8
Mon	19		1st Sh. E. 7 35 2nd Oc. D. 10 21	10 13.5
Tues	20	16 7 ☾ Moon's Last Quarter		
		14 59 Occultation of 16 Tauri (54)		
		15 35 Reappearance of ditto		
		15 3 Occultation of 19 Tauri (5)		10 9.1
		16 14 Reappearance of ditto		
Wed	21	15 37 Occultation of 20 Tauri (5)		
		16 10 Reappearance of ditto		
Thur	22	15 Conjunction of Uranus and Sun	2nd Tr. E. 8 18 2nd Sh. E. 9 37	10 4.8
Fri	23		3rd Tr. I. 12 20	9 56.1
Sat	24		4th Sh. E. 7 24 1st Tr. I. 11 57 1st Sh. I. 12 41	9 51.8
		14 31 Occultation reappearance of B.A.C. 2854 (6)		
Sun	25	21 Conjunction of Moon and Venus $0^{\circ} 9' S.$	1st Oc. D. 9 16 1st Ec. R. 12 19 18	9 47.5
Mon	26		1st Sh. I. 7 10 1st Tr. E. 8 44 1st Sh. E. 9 30 2nd Oc. D. 12 39	9 43.2
		17 59 ● New Moon	3rd Ec. R. 8 45 54	9 38.9
Wed	28	8 Conjunction of Moon and Mars $3^{\circ} 59' N.$	2nd Tr. I. 7 41 2nd Sh. I. 9 18 2nd Tr. E. 10 38 2nd Sh. E. 12 14	9 34.7
		3 Conjunction of Moon and Mercury $0^{\circ} 46' S.$		9 30.4
Fri	30			9 26.2
Sat	31			9 21.9
SEPT.				
Sun	1		4th Oc. R. 7 13 4th Ec. D. 10 53 34 1st Oc. D. 11 3	9 17.7

JUPITER.

1878. oh. Gr.	Angle of position of J's axis.	Latitude of Earth Sun above J's equator.		Equat. diameter.	Defect of illumi- nation.
Aug. 3	345°56	—0°58	—0°65	47°66	0°01
13	346°02	0°60	0°60	47°21	0°06
23	346°42	—0°62	—0°55	46°47	0°12

The assumed First Meridian passes the middle of the illuminated disk at the following times :

1878.	G. M. T.				
	h. m.		h. m.		
Aug. 1	6 13.7	Aug. 12	0 13.6	Aug. 22	8 18.8
	16 9.1		10 9.0		18 14.2
2	2 4.4		20 4.4	23	4 9.7
	11 59.8	13	5 59.8		14 4.1
	21 55.2		15 55.2	24	0 0.5
3	7 50.6	14	1 50.6		9 55.9
	17 45.9		11 46.0		19 51.3
4	3 41.3		21 41.4	25	5 46.8
	13 36.7	15	7 36.8		15 42.2
	23 32.1		17 32.2	26	1 37.6
5	9 27.5	16	3 27.6		11 33.0
	19 22.8		13 23.1		21 28.5
6	5 18.2		23 18.5	27	7 23.9
	15 13.6	17	9 13.9		17 19.3
7	1 9.0		19 9.3	28	3 14.8
	11 4.4	18	5 4.7		13 10.2
	20 59.7		15 0.1	23	5.6
8	6 55.1	19	0 55.5		9 1.1
	16 50.5		10 50.9		18 56.5
9	2 45.9		20 46.3	30	4 52.0
	12 41.3	20	6 41.8		14 47.4
	22 36.7		16 37.2	31	0 42.8
10	8 32.1	21	2 32.6		10 38.3
	18 27.5		12 28.0		20 33.7
11	4 22.9		22 23.4	Sept. 1	6 29.2
	14 18.3				16 24.6

A. MARTIN

A. MARTH.

THE INNER SATELLITES OF SATURN.

Major and minor semi-axes *a* *b* of the apparent orbits of the inner satellites :

	Mimas.		Enceladus.		Tethys.		Dione.		Rhea.	
	<i>a</i>	<i>b</i>	<i>a</i>	<i>b</i>	<i>a</i>	<i>b</i>	<i>a</i>	<i>b</i>	<i>a</i>	<i>b</i>
Aug.										
3.	28°75	2°36	36°90	3°02	45°69	3°74	58°52	4°80	81°74	6°70
13.	29°15	2°28	37°41	2°92	46°32	3°62	59°33	4°64	82°86	6°47
23.	29°48	2°16	37°83	2°77	46°84	3°43	60°02	4°40	83°81	6°14

Approximate Greenwich times of some of the conjunctions of the satellites with the ends of the ring, and also of some of their greatest elongations.—V. p. 189 :

1878. Gr. M. Time.

	h.		
Aug. 1	10.6	Tethys.	w.
	11.6	Encel.	e.
	13.4	Dione.	w.
	13.7	Mimas.	w.
2	9.2	Tethys.	e.
	9.8	Dione.	nf.
	12.3	Mimas.	w.
	15.5	Encel.	sf.
3	10.7	Dione.	nf.
	10.9	Mimas.	w.
	12.9	Encel.	w.
	15.6	Tethys.	sp.
4	9.6	Mimas.	w.
	10.3	Encel.	nf.
	11.0	Rhea.	np.
	14.3	Tethys.	nf.
5	9.3	Encel.	sf.
	9.5	Rhea.	w.
	12.9	Tethys.	sp.
	14.2	Encel.	e.
6	15.9	Dione.	e.
	11.6	Tethys.	nf.
	11.6	Encel.	sp.
	12.3	Dione.	np.
7	10.2	Tethys.	sp.
	10.6	Encel.	np.
	13.2	Dione.	sp.
	15.5	Encel.	w.
8	15.6	Rhea.	e.
	8.9	Tethys.	nf.
	9.6	Dione.	e.
	12.9	Encel.	nf.
9	14.1	Rhea.	nf.
	15.3	Mimas.	e.
	11.9	Encel.	sf.
	13.9	Mimas.	e.
10	14.6	Tethys.	sf.
	9.3	Encel.	w.
	12.5	Mimas.	e.
	13.3	Tethys.	np.
11	14.2	Encel.	sp.
	14.8	Dione.	sf.
	11.2	Mimas.	e.
	11.9	Tethys.	sf.
12	13.2	Encel.	np.
	15.7	Dione.	nf.
	10.6	Tethys.	np.
	10.6	Encel.	e.
13	12.1	Dione.	w.
	15.6	Encel.	nf.
	9.2	Tethys.	sf.
	11.7	Rhea.	np.
14	14.6	Encel.	sf.
	9.4	Dione.	nf.

1878. Gr. M. Time.

	h.		
14	10.2	Rhea.	w.
	11.9	Encel.	w.
	15.6	Tethys.	w.
15	8.7	Rhea.	sp.
	9.3	Encel.	nf.
	14.3	Tethys.	e.
16	12.9	Tethys.	w.
	13.2	Encel.	e.
	14.6	Dione.	e.
17	10.6	Encel.	sp.
	10.9	Dione.	sf.
	11.6	Tethys.	e.
18	14.2	Mimas.	w.
	14.8	Rhea.	nf.
	9.6	Encel.	np.
19	10.2	Tethys.	w.
	11.8	Dione.	sp.
	12.8	Mimas.	w.
20	14.6	Encel.	w.
	8.2	Dione.	e.
	8.9	Tethys.	e.
21	11.4	Mimas.	w.
	11.9	Encel.	nf.
	10.0	Mimas.	w.
22	10.9	Encel.	sf.
	15.3	Tethys.	sp.
	13.3	Encel.	sp.
23	13.4	Dione.	sf.
	13.9	Tethys.	nf.
	12.3	Encel.	np.
24	12.4	Rhea.	np.
	12.6	Tethys.	sp.
	14.3	Dione.	nf.
25	9.6	Encel.	e.
	10.7	Dione.	w.
	10.9	Rhea.	w.
26	11.2	Tethys.	nf.
	14.6	Encel.	nf.
	9.3	Rhea.	sp.
27	9.9	Tethys.	sp.
	13.6	Encel.	sf.
	15.8	Mimas.	e.
28	8.0	Dione.	nf.
	8.5	Tethys.	nf.
	10.9	Encel.	w.
29	14.4	Mimas.	e.
	15.6	Tethys.	np.
	15.9	Encel.	sp.
30	15.9	Dione.	np.
	8.3	Encel.	nf.
	13.0	Mimas.	e.
31	14.3	Tethys.	sf.
	14.9	Encel.	np.
	15.5	Rhea.	nf.

1878. Gr. M. Time.

	h.		
27	11'6	Mimas.	e.
	12'3	Encel.	e.
	12'9	Tethys.	np.
	13'2	Dione	e.
28	9'6	Dione.	np.
	9'6	Encel.	sp.
	10'2	Mimas.	e.
	11'6	Tethys.	sf.
29	8'6	Encel.	np.
	8'8	Mimas.	e.
	10'2	Tethys.	np.
	10'5	Dione.	sp.

1878. Gr. M. Time.

	h.		
	13'6	Encel.	w.
30	8'8	Tethys.	sf.
	11'0	Encel.	nf.
31	10'0	Encel.	sf.
	13'1	Rhea.	np.
	14'9	Encel.	e.
	15'3	Tethys	w.
	15'7	Dione.	w.
Sept. 1	11'6	Rhea.	w.
	12'0	Dione.	sf.
	12'3	Encel.	sp.
	13'9	Tethys.	e.

On August 14 Iapetus reaches its greatest western elongation, and on Sept. 2 its superior conjunction, south of the planet.

A. MARTH.

MINOR PLANET JUNO.

The minor planet Juno, the third of the series, will come into opposition on the 28th of the month.

August	Rt. Ascen.			Decl.		
	h.	m.	s.	S.	°	'
1	12	38	41	S.	0	4 35
15	22	31	9	S.	1	44 54
25	22	23	58	S.	3	21 30

NEW MINOR PLANET (188).

(From *Astronomische Nachrichten*, No. 2205.)

Peters, of Clinton, New York, announces the discovery by Peters, at Clinton, on the 26th of June, 1878, of a planet of the 12th mag.

R. A. 15h. 37m. Decl. 16° 8' Daily motion 6 N.

Berlin: June 27.

V. KNORRE.

Books Received.—Astronomical observations to the end of 1877, at the Private Observatory of J. G. Barclay, Esq. Vol. IV. London: Williams & Norgate.—Mondcharte in 25 Sectionen und 2 Erläuterungstafeln. Von W. G. Lohrmann, mit verschreibendem Text von Dr. J. F. Julius Schmidt. Leipzig: J. A. Barth.—The Physical System of the Universe. By S. B. J. Sketchley, F.G.S. London: Isbister & Co. 1878.—The Moon. By R. A. Proctor. Second Edition: Longmans, Green & Co. 1878.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To June, 1878.

Compton, A. J. S.
Horner, Rev. J.

To Sept, 1878.

Jonas, W. E.
Elliot, H.

To Dec., 1878.

Glaisher, J.
Numsen, W. H.
Remington, Major.
Schafuric, Prof.

To June, 1879.

Gooch, Miss E.

The *Astronomical Register* is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings per Quarter, payable in advance**, by Penny postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, Angel Court, Throgmorton Street, E.C., not later than the 15th of the Month.

The Astronomical Register.

No. 189.

SEPTEMBER.

1878.

TRANSIT OF MERCURY OF MAY 6, 1878.

BY S. P. LANGLEY.

The following observations made during the transit of Mercury, yesterday, have some interest from the inferences to be drawn from them as to physical phenomena; and to devote the opportunity more wholly to this object, no measures of precision were attempted, beyond noting the times of ingress and egress. The principal instruments were the equatorial refractor of thirteen inches (used in the early part of the day with nine inches effective aperture), and a polarising solar eye-piece, which dispenses altogether with any dark glass, and presents objects in their natural colours and relative brightness.

I had the fortune, at ingress, of an unusually blue and transparent sky, and aided by this, saw with the polarising eye-piece the entire disk of Mercury *outside* the sun about one-half a minute before first external contact. Presumably it might have been seen even earlier, had not time been lost in searching for it, through lack of means to designate the precise position-angle, the position filar-micrometer not being adaptable to this eye-piece. After a pause to verify the reality of the phenomenon by revolving the eye-lens, etc., the chronograph key was struck at 21h. 52m. 32.45s. Allegheny mean time, to record the observation. As this was really made earlier, and the disk was seen throughout its circumference, it seems clear that the coronal background is bright enough to produce this effect at least

VOL. XVI.

fifteen seconds of arc from the solar limb, and in spite of the atmospheric glare.

As a partial substitute for the filar-micrometer, there was in the field a glass reticule, ruled (by Prof. Rogers, of Harvard) in squares whose sides represented here $15''.3$, and this enabled—not a measurement—but a fair comparison to be made of the apparent size of the planet before and after it entered on the sun. The contrast was striking, as on a background very little brighter than itself its diameter was, if anything, greater than one of the sides of these squares, while as soon as it entered on the sun it seemed to shrink by more than one-fifth of this. First external contact was noted on the chronograph at 21h. 52m. 50.43s. First internal contact was noted when the sunlight could be seen unmistakably between the disk and limb at 21h. 55m. 47.25s. These entries I believe to have been made in both cases nearly two seconds late. The limb just at second contact was steady. I saw no “black drop” or “ligament.”

As the disk advanced on the sun it was closely scrutinised, without at any time any “bright point” or “annulus” being seen. These appearances, resting as they do on much testimony, particularly the unimpeachable evidence of Mr. Huggins, I was prepared to expect, but fruitlessly looked for with powers varying from 120 to 800 throughout the day, with the polarising eyepiece, and also by projection of the image. The phenomenon may depend for its visibility on exceptionally good definition, which Mr. Huggins* appears to have had; that here was fairly, though not unusually, good. The darkest part of the planet was the centre, the edges being decidedly less grey. The cause of this gradation came out very clearly in forming a very enlarged image for projection, being plainly due in most part to minute and rapid atmospheric tremor. In moments of best definition the surface became of a nearly uniform shade throughout.†

The planet has been almost uniformly described as looking “black” in transit, but in the instrument I use (the objective of which was corrected by Mr. Alvan Clark), it certainly does not look black. The colour is decidedly less red than that of spot nuclei, being grey, slightly inclining toward a blue, like that of the spectrum between F. γ G. (It may be that this bluish cast comes from the secondary spectrum of the objective). The average light from the disk in transit is very considerable,

* *Monthly Notices R. A. S.*, vol. xxix., p. 26.

† I presume that even in absolutely perfect definition there would be theoretically a slight gradation due to another cause, *i.e.*, to the greater effect of the edge of the planet's disk of the inflection, referred to in a subsequent paragraph.

being not much less than that of some nuclei. No spots were present for comparison, but being engaged in photometric determinations of these and other parts of the solar surface, I was provided with means of comparing Mercury with tints which had previously been contrasted with sun-spots under like conditions. Absolute photometric determinations of the apparent light from Mercury in transit were attempted by projecting a greatly enlarged image (its actual diameter was three-quarters inch as projected), on a white surface in a dark camera attached to, and moving with, the equatorial. Direct measurements with a Jamin photometer were unsatisfactory. Subsequently, by another method, a trustworthy value was fixed for a minimum. It was thus found that the light actually received on the paper, apparently from the so-called "black" body of the planet, at any rate exceeded eight per cent, of that from direct sunlight, and measures taken by the thermopile and galvanometer showed that heat was coming from the same direction.

It need hardly be said that it is impossible that Mercury itself should be radiating heat and light in any such degree. Accordingly I take these numbers as representing (with some possible allowance for instrumental causes) the minimum effect we can assign to our own atmosphere in deflecting the solar radiation, a subject on which data have been hitherto desirable. It is evident, for instance, that from the facts here stated we can estimate, photometrically, the intrinsic brightness of the corona, since it was undoubtedly this, acting as a background, which enabled the planet, though itself involved to a calculable extent in atmospheric glare, to be seen before it reached the solar limb.

The observations were interrupted by haze in the afternoon, and egress was so nearly invisible that the apparent times of contact are not worth giving.

Allegheny Observatory:

May 7, 1878.

(Reprinted from the *American Journal of Science and Arts*,
Vol. XV., June, 1878.)

ROGER BACON.

"The life of Roger Bacon almost covers the thirteenth century. From Oxford . . . he passed to the University of Paris, where his whole heritage was spent in costly studies and experiments. 'From my youth up,' he writes, 'I have laboured at the sciences and tongues. I have sought the friendship of all men among the Latins who had any reputation for knowledge.

I have caused youths to be instructed in languages, geometry, arithmetic, the construction of tables and instruments, and many needful things besides.' . . . 'Without mathematical instruments no science can be mastered,' he complains afterwards, 'and these instruments are not to be found among the Latins, and could not be made for two or three hundred pounds. Besides, better tables are indispensably necessary, tables on which the motions of the heavens are certified from the beginning to the end of the world without daily labour, but these tables are worth a king's ransom, and could not be made without a vast expense. I have often attempted the composition of such tables, but could not finish them through failure of means, and the folly of those whom I had to employ.' . . . It is probably of himself that he speaks, when he tells us that 'the science of optics has not hitherto been lectured on at Paris or elsewhere among the Latins, save twice at Oxford.' It was a science on which he had laboured for 10 years. . . . After 40 years of incessant study, Bacon found himself, in his own words, 'unheard, forgotten, buried.' He seems at one time to have been wealthy, but his wealth was gone. 'During the 20 years that I have specially laboured in the attainment of wisdom, abandoning the path of common men, I have spent on these pursuits more than £2,000, not to mention the cost of books, experiments, instruments, tables, the acquisition of languages, and the like. Add to all this the sacrifices I have made to procure the friendship of the wise, and to obtain well-instructed assistants.' . . . The '*Opus Majus*' is alike wonderful in plan and detail. . . . The development of his scheme is on the largest scale; he gathers together the whole knowledge of his time in every branch of science which it possessed, and as he passes them in review he suggests improvements in nearly all. His labours, both here and in his after works, in the field of grammar and philology, his perseverance in insisting on the necessity of correct texts, of an accurate knowledge of languages, of an exact interpretation, are hardly less remarkable than his scientific investigations. But from grammar he passes to mathematics, from mathematics to experimental philosophy. Under the name of mathematics was included all the physical science of the time. 'The neglect of it for nearly 30 or 40 years,' pleads Bacon passionately, 'hath nearly destroyed the entire studies of Latin Christendom. For he who knows not mathematics cannot know any other sciences; and, what is more, he cannot discover his own ignorance or find its proper remedies.' Geography, chronology, arithmetic, music, are brought into something of scientific form, and the same rapid examination is devoted to the question of climate, to hydrography, geography,

and astrology. The subject of optics, his own especial study, is treated with greater fulness; he enters into the question of the anatomy of the eye, besides discussing the problems which lie more strictly within the province of optical science. In a word, the 'Greater Work,' to borrow the phrase of Dr. Whewell, is 'at once the *Encyclopædia* and the *Novum Organum* of the thirteenth century.' . . . Such a work was its own great reward. From the world around Roger Bacon could look for, and found, small recognition. . . . 'Unheard, forgotten, buried,' the old man died as he had lived, and it has been reserved for later ages to roll away the obscurity that had gathered round his memory, and to place first in the great roll of modern science the name of Roger Bacon."—*Green's "Short History of the English People,"* pp. 133—136.

REVIEWS.

Annual Report of the Director of Harvard College Observatory. Presented to the Visiting Committee, November 26th, 1877. By Professor Edward C. Pickering.

Annals of the Astronomical Observatory of Harvard College. Vol. X. Observations made with the Meridian Circle during the years 1871 and 1872, under the direction of the late Joseph Winlock, A.M., Phillips Professor of Astronomy and Director of the Observatory. By William A. Rogers, A.M., Assistant Professor of Astronomy in the Observatory. pp. lxxxix. and 239. Cambridge: Press of John Wilson and Son. 1877.

This observatory possesses two instruments of the largest size and finest quality, an equatorial of 15 inches aperture, and a meridian circle whose telescope has an aperture of 8 inches. Professor Pickering states that all the work heretofore done with the last mentioned has been dependent on a standard catalogue of stars observed at the Pulkova observatory, which is provisionally accepted as correct. One of the greatest defects of modern astronomical observations is the systematic error thus engrafted from one catalogue into another. It is accordingly proposed as soon as possible to prepare a catalogue wholly independent of all previous observations. The report proceeds to particulars of *Books and MS. and Publications.* During the past year Vol. X. of the *Annals* has been published and distributed, but a large amount of material is not yet printed. Under the head of *Scientific Work*, we find that 400 measurements have been made of the brightness of the outer satellite of Mars, and about 70 of the inner satellite. Two or three wholly independent methods have been employed, and will give the true brightness with considerable precision. It is believed that no accurate measures of the brightness of these bodies have been made elsewhere. The satellites of Jupiter and Saturn have been similarly measured. Over a hundred measures have been obtained of Hyperion, the faintest of the satellites of Saturn. To simply see this satellite is regarded as a severe test to any but the very largest telescopes. Several asteroids have been compared in brightness with Mars and Saturn. These comparisons will furnish the first satisfactory means of estimating the actual size of the smaller planets. The two

components of several double stars have been compared. Some have been selected in which one component is extremely minute, so that these measurements will give a standard with which observers hereafter can compare other faint stars, and thus reduce the present vagueness of the higher magnitudes to comparative precision. Besides the measures of the brightness of the satellites of Mars, a large number of determinations of their positions were made by Mr. Waldo. This set is believed to be second only to that made by Prof. Hall with the 26 inch telescope at Washington. The red colour of Mars was found not to be possessed by the outer satellite.

A beginning has been made of a direct comparison of the light of the sun and stars, the latter being observed during the day with the full aperture of 15 inches, while the sun is reduced, first by refraction through a small quartz sphere, and then by observing it in a small auxiliary telescope. It is proposed thus to reduce all our photometric measurements to a common unit—the light of the sun. The observations of the Cambridge zone have been continued with the meridian circle. This is the share taken by the observatory in the determination of the accurate position of all stars of the first nine magnitudes contained in the northern hemisphere. These form a large part of Argelander's great catalogue called the *Durchmusterung*, the revision of which has recently been undertaken by several observatories acting in concert. The only portion of this work which is being done in this country is the Cambridge zone, and includes the stars between 50° and 55° north. The report goes on to notice observations of Mars at the recent opposition, and of comparison stars for Mr. Gill, for the determination of the solar parallax by means of the planet Ariadne; also an arrangement with the U. S. Coast Survey for the determination of the position of all stars in the northern hemisphere visible to the naked eye, whose positions have not recently been determined with the greatest precision. Prof. Pickering remarks, It may be taken for granted that any work is valuable which, by comparatively few observations, will increase the value of a previous much larger series of observations. Two memoirs offer a tempting field in this respect, the observations of variable stars by Argelander, and the magnitudes of the stars given in the *Durchmusterung*. The first of these gives over thirty thousand determinations of the brightness of the most important variable stars, observed during thirty years, by one of the most skilful of observers. But the results are only comparative, and at present almost valueless. A portion of these observations has been reduced by Mr. C. H. Metcalf, student at this observatory, and it is proposed to measure the brightness of the comparison stars, and thus determine the true brightness of the variables, as given by this immense mass of observations. The *Durchmusterung* contains eye-estimates of the brightness of 300,000 stars, each observed twice. But we have no knowledge of the true brightness of the fainter magnitudes. It is proposed to measure the true value of this scale, so that the brightness of the fainter stars may be known in terms of the brighter.

Time Service. One of the great benefits conferred on mankind by the science of astronomy is in providing an accurate standard of time. . . . Active measures are now being taken to secure a wider knowledge of our time-signals, &c., &c. After notices of *Instruments, Buildings and Grounds*, we find that four assistants are attached to the observatory: Professor Rogers, Mr. Searle, Mr. Waldo, and Mr. Upton. To Professor Rogers has been assigned the entire charge of the new meridian circle, and the large amount and excellent quality of the work of this instrument is due almost wholly to him. Mr. Searle, besides assisting in the equatorial

observations, and having charge of the meteorological observations, attends to most of the clerical work of the observatory. The Time Service has been in charge of Mr. Waldo. . . . Mr. Upton has rendered valuable assistance in the equatorial observations, and in reducing the work of the meridian circle. We regret to find that the salaries of the assistants are far below what they should be. The *Financial Condition* of the observatory is not yet what could be desired. From want of means little work of a high order has of late years been done with the superb equatorial. Although, by the erection of still larger telescopes at Washington and Chicago, the observatory has lost its former pre-eminence in this respect, yet the best work is not always done with the largest instruments. There is scarcely any purpose, Professor Pickering goes on to say, for which our telescope is not sufficiently powerful, and an endowment by which it could be constantly employed would more than compensate for its smaller size. We entertain strong hopes that the convincing observations of the Professor will before long cause the present deficiencies to be remedied. He says, "To bring the observatory to the highest state of efficiency with its present equipment, about six thousand dollars a year would be required, in addition to our present income."

Appendix A is a report on the 15-inch equatorial, from which we copy the following interesting particulars. In laying out a course of work for this instrument, two principles are to be considered: First, to avoid a duplication of the work done elsewhere with other large telescopes; and, Secondly, that the observations shall be of such a kind that they cannot be made equally well with a much smaller instrument. The principal classes of work fulfilling the last of these conditions are as follows: 1. Descriptions or drawings of the appearance of the heavenly bodies. A full share of this class of work has already been done at this observatory. Moreover, in the present state of science it has less value than in former years. 2. Zone observations.—Three volumes of the *Annals* have been devoted to measuring the position of the stars between 0° and 1° north. Dr. Peters is engaged in the determination of the stars near the ecliptic, with the large telescope at Clinton. 3. Asteroids.—The discovery of these bodies is the natural consequence of 2. The telescopes at Clinton and Ann Arbor are mainly employed in this work. 4. Micrometric measurements.—The great telescopes at Washington, Chicago, and Cincinnati, are used almost entirely for these observations. 5. Spectroscopy.—Efforts have been made to secure a large telescope for Princeton College for this work. 6. Photography.—The telescopes of Mr. Rutherford and Dr. Draper, are largely used for photographic purposes. 7. Solar phenomena.—The telescope of the Allegheny Observatory is devoted to this work. With a sufficient number of observers, either of the other classes of observations may be carried on simultaneously with it. 8. Photometry.—This subject offers a field almost wholly unexplored with large telescopes, either in this country or abroad. It has, therefore, been selected as that to which the greater portion of the time of our telescope will be devoted. The problem of astronomical photometry may be stated in its most general form to be as follows: To determine the brightness of all the heavenly bodies, so that all may be compared with a single standard. The photometric measurements heretofore published have been made almost entirely upon the planets and brighter stars. Our knowledge of the light emitted by the satellites and fainter stars is confined to estimates by the eye, according to an arbitrary scale, which differs with each observer. . . . A photometer was made by attaching to the eye-piece of the telescope a Nicol prism and double-image prism.

By the last of these two, images of any object are formed, and by turning the Nicol prism through an angle given by a graduated circle, the relative brightness of the images may be altered in any desired proportion. Accordingly, if turned towards a double star, the faint image of the bright component may always be reduced to equality with the bright image of the faint component. Various double-stars were thus measured, and the satellites of Jupiter were compared with one another. The extreme faintness of the satellites of Mars, rendered the determination of their brightness and position very difficult, for they could not be seen when the planet was in the field, but a small piece of coloured glass through which Mars could be seen enabled Mr. Waldo to take a series of 322 measurements on 15 different nights. This series is believed to be surpassed only by the series taken by Prof. Hall at Washington, where the large size of the telescope used (giving over three times the light of our instrument) greatly facilitated the observations.

The first attempt to measure the light of the satellites was made on August 27, by allowing Mars to shine through a very small hole placed in the focus of the telescope, and comparing this with the satellite. To obtain a hole sufficiently minute and circular was a matter of no small difficulty. It was at length accomplished by Mr. Van Woerd, Superintendent of the American Watch Company, by the construction of a hole one eighteen-hundredth of an inch in diameter, and so nearly circular that the various diameters, including errors of measurement, only differed one hundred-thousandth of an inch! Even this hole, however, represented a satellite seventy miles in diameter, and shone out as a bright star when Mars was placed behind it. One hundred and eleven comparisons of this and other holes with the satellite were obtained. . . . A wholly different photometer was next tried. A small auxiliary telescope was attached to the large instrument, and by means of prisms the light of Mars was reflected so that its image formed by the little telescope should be seen by the side of the image of the satellite formed by the large telescope. . . . We then had two stars on fields which were first rendered equally bright, then matched in colour, and the image of Mars was then brought to precisely the same brightness as the satellite. Two hundred and eighty-two comparisons of the outer, and seventy-two of the inner satellite were thus made. To compare the two satellites directly with one another, still a different method was tried. One, two, and three plates of thin glass were successively interposed between the eye and the inner satellite. Its light was thus so far reduced as to render it equal to, or fainter than, the outer and smaller satellite. These observations have not yet been wholly reduced: it is therefore impossible to give the final conclusions. The best idea of the light of a satellite is obtained by giving the diameter to which the primary must be reduced to render it no brighter than the satellite, or the diameter of the latter, if it reflected light in the same proportion as the planet. This is, in fact, probably the only estimate we can ever make of the true diameter of these bodies. An approximate reduction of the measurements of the outer satellite, comparing its light with that from the holes, gives its equivalent diameter at about 5.9 miles. The result of the other comparison is about 5.4 miles. This agreement is all that could be desired. The observations of the inner satellite give its diameter as 6.5 miles. The direct comparison of the two gives their relative diameters as in the ratio of 10 to 9. These figures will be somewhat altered in the final reduction. As the darker colour of the outer satellite somewhat diminishes its light, it is probably safe to call it about 6 miles in diameter, and the inner satellite 7 miles. The labour involved in these measurements was very great. . . . Still another

photometer was devised, by which Saturn's satellites, minute companions to bright stars, and several asteroids were measured.

Appendix B is a report by Prof. W. A. Rogers, of the work done with the meridian circle during the year. The whole number of observations is 5,285, but since the instrument was mounted in 1870, 40,600 observations of stars, &c., have been made, including 18,000 zone observations. Appendix C is Mr. L. Waldo's report on the Time Service. The standard mean time clock by Bond (394) is a beautiful specimen of horological workmanship. As a specimen of its performance, between March 8, 1872, and July 22, 1872, the difference between its greatest and least rates was 0.4s., although exposed to a variation of temperature of 60° F. All observations are referred to the sidereal clock Frodsham, 1327. The "Russian Transit" used for the observations has an aperture of 2.75 in., and stars of the second magnitude may be observed with it in the daytime. The error in transmitting signals may reach 0.4s. in extreme cases, but ordinarily the clock-beats are correct within 0.15s.

From this highly interesting report of Prof. Pickering, we pass on to the volume of *Annals*, &c., which contains the first portion of the work with the new meridian circle under the direction of Prof. Winlock. The observations were made by Assistant Prof. William A. Rogers, aided by Mr. Augustus McConnell. Prof. Rogers has also superintended their entire reduction, and has prepared this volume. The skill of the observer and excellence of the instrument are shown by the probable errors given on page lxxv. From this it appears that the probable error of a single observation of the declination of a star in the zenith, from the reading of two microscopes, is $\pm .48$. The probable error of a single measurement of R.A. is $\pm s. .024$. All the work is differential, the preliminary catalogue of the Gesellschaft being taken as a standard. An independent determination of the proper motions has been made by comparing the results of twenty-seven different catalogues. They were selected so as to form four groups, having as mean dates about 1755, 1800, 1837, and 1870. There is a description of the meridian circle, with plates. Of the 564 stars observed, 289 are found in the catalogue furnished by the "Astronomische Gesellschaft," for the reduction of the zone observations undertaken under its auspices. These primary stars furnish the positions upon which the remaining 275 secondary stars depend. Cat. I from p. 203 to p. 211, contains the mean positions of the primary stars for 1872.0 derived from the observations, &c. Cat. II., from p. 214 to p. 227, contains mean positions of the secondary stars for 1872.0 derived from the observations, &c. Cat. III., from p. 231 to p. 240 contains the mean Right Ascension for 1868.0 of the stars of the catalogue observed with the old transit circle, &c. The observations were nearly all made by Mr. E. P. Austin, and the reductions were either made by him or under his direction.

This very valuable volume is worthy of the long recognised eminence of the Harvard College Observatory. A splendid instrument, made the most of by skillful observers, has been the means of enriching sidereal astronomy with another of those precious works which aim at presenting with the last degree of precision, and the elimination as far as possible of every kind of error, the places of the stars, and their proper motions. Those who may possess it will appreciate the distinctive features it possesses, and especially the comparison with a large number of previous standard catalogues, indeed, we fancy, of all that are of importance. It is admirably printed, from funds resulting from the will of Josiah Quincy, jun., who died in April, 1775. We think our readers will value the above extracts (we have left much unnoticed), and rejoice in the evidence they

afford that whilst astronomy is already so vastly indebted to the institutions, observers, and mathematicians of the U. S., the prospects of the science there in every department are full of hope and promise.

Astronomical Observations made at the Royal Observatory, Edinburgh. By Piazzzi Smyth, F.R.S.E., F.R.A.S., F.R.S.S.A., &c. Vol. xiv., for 1870-1877. Published by order of Her Majesty's Government. Edinburgh. Printed by Neill & Co., 1877. pp. xvii.—641 and 57, and 22 plates.

From a lithographed letter accompanying this fine and beautifully printed volume, we extract the following :—"It is believed that immense stores of useful, though as yet not fully used star-place observations exist in various observatories and libraries of scientific societies throughout Europe. But as they are generally calculated for different years, and star-places, when reckoned in R.A. and N.P.D. vary excessively from year to year through 'Precession,' the identity of the smaller and more numerous nameless stars does not always immediately appear. Hence some of them have been undoubtedly observed over and over again by different observers, without the fact having been recognised, or the variation of place from date to date having been compared with theory, and declared either normal or anomalous, and by how much and at what time. Under these circumstances, it is hoped that the form adopted in printing this Edinburgh Catalogue, and leaving blank lines when further observations are required for the history of a star's path in the sky, will be found to offer a convenient and common meeting ground, whereon astronomers of all nations, if they have observed any of those stars in past years, may easily and readily introduce their results, and compare them equally with ours, against one given theory at certain stated epochs; those epochs having been chosen as apparently most suitable for securing unanimity of action in the international astronomy of the present age of the world." This idea appears a good one, and likely to be taken up by astronomers.

The chief instruments of the observatory are the 8-foot transit instrument, and the 6-foot mural circle, described as very antiquated and frail. There are sidereal and mean-time clocks under electric control, the latter extending from the observatory over the whole city of Edinburgh. The reflecting equatorial of large aperture is still in the hands of Mr. Grubb of Dublin. There are two assistants, Mr. Alexander Wallace, M.A., Ed. (acting since 1834), and Mr. Thomas Heath, B.A., T.C.D. (since 1874), both of whom are highly praised. The largest part of this volume is occupied by the Edinburgh star-place catalogue and ephemeris for 1830 to 1890 A.D. (the first four hours in R.A.), the final results of which are compared with the observations of all other good observatories and accessible modern catalogues. It is believed that no R.A. of any zodiacal star is erroneous by one-tenth of a second of time, and no N.P.D. by a second and a-half of space. Great errors have been discovered in the B. A. catalogue; some of them rapidly increasing, showing the necessity of preparing a new catalogue.

Next we have observations of the earth-thermometers from 1870 to 1876, and a review of the whole series for 39 years, with illustrations, and general results deduced, of considerable interest. There were three great waves of heat in 1846, 5,—1858, 0, and 1868, 7, and Professor Smyth thinks that another may be expected in 1879, 5, within limits of half a year each way; also that a somewhat cold wave may be expected about 1878, 0 (part of it already felt this summer); and another such about 1881, 4, each of them within limits of three quarters of a year on

either side. But he thinks that we may be more confident of the dates, than the degrees or intensities of the temperatures which will arrive. We must banish, he says, all idea of the sun-spots themselves being active solar agents in producing such cyclic changes in temperature; yet both the spots and the temperature waves may be the product of some other phenomenon, and this he suggests (as in his paper to the Royal Society in 1870) may be that occasional evolution of gaseous matter in the sun, producing the now well known red prominences. Such bursting forth of incandescent hydrogen matter is, of course, favorable to the increased radiation of solar heat, and has been observed highly to increase the radiation of other suns than ours.

Under the head of *Spectroscopy*, we have *Phenomenal Spectroscopy*, 1870—74, Edinburgh and Palermo, plates 1 to 13. *Meteorological Spectroscopy*, 1872—77, Edinburgh, Toulouse, &c., plates 14 and 15. *Solar Spectroscopy*, 1877, Edinburgh and Lisbon, plates 20, 21 and 22. Some of the illustrations under the first head are beautiful coloured plates of the zodiacal light, of auroras, of twilight, and phosphorescence. Meteorological spectroscopy has now become a comparatively settled method of foreseeing rain, and is reduced to an almost momentary look with a waistcoat-pocket spectroscope at the lower part of the sky, and then an entry of two estimated numbers in appropriate columns of a meteorological journal. Professor Smyth describes the manner in which he was led to discover this method, and gives instructions concerning it. Testimony from Madras shows the greater visibility of all the telluric bands, but more especially of the rain-band near D, in India, than in Britain. Every occasion of special strength in the rain-band was invariably followed before night by thunder and lightning, as well as rain. A spectroscope for these observations is made by Mr. Adam Hilger, of 192, Tottenham Court Road, who has used the spectroscope for detecting the rain-band with much enthusiasm and success. Under the head of *Solar Optical Spectroscopy*, the Professor describes a superior instrument, made at his own expense, in 1876, and compares the results of observations with it with Angstrom's magnificent Normal Solar Spectrum Map, with another by the Royal Society, and with Brewster and Gladstone's Solar Spectrum, and discusses some disputed points. The volume concludes with Reports on the Royal Observatory, Edinburgh, and other documents, from 1873 to 1877.

In various respects Professor Piazzi Smyth complains, with justice, of the "poverty-stricken" condition of the Edinburgh Observatory; and it is in the highest degree creditable to him and his assistants, that under so many disadvantages they have been able to put forth this admirable volume of observations,—a proof of what talent and laboriousness like theirs might produce with more adequate means, instrumental and pecuniary. A little fraction of large sums of public money, sometimes wasted in various ways, would suffice to place this Royal Observatory on a proper footing. We must hope that those in whose power it lies to rectify the present unsatisfactory state of things will not much longer delay giving to it their serious attention.

Myths and Marvels of Astronomy. By Richard A. Proctor. London: Chatto and Windus, Piccadilly. 1878. pp. 363.

The contents of this entertaining and instructive work are as follows: Astrology—The Religion of the Great Pyramid—The Mystery of the Pyramids—Swedenborg's Visions of other Worlds—Other Worlds and Other Universes—Suns in Flames—The Rings of Saturn—Comets as Portents—The Lunar Hoax—On some Astronomical Paradoxes—On

some Astronomical Myths—The Origin of the Constellation Figures. We must confine our notice to one or two only of the above papers. Mr. Proctor regards the builders of the Great Pyramid as ingenious mathematicians who had made some progress in astronomy, but he does not admit anything supernatural in the construction and object of the edifice, such as is maintained by Professor Piazza Smyth and others, and his reasons seem to us difficult to set aside. It is a subject we have looked at from time to time for years past, certainly with no *a priori* objection to the "inspiration theory," startling as it may appear, but rather inclined, if possible, to become one of the "Pyramidalists," as Mr. Proctor calls them. In this direction the reading of his papers has certainly not helped us, though somewhat bewildered at the extraordinary mass of coincidences we seem obliged to admit on any other hypothesis. But according to the author, the number of these, under the circumstances, is no cause of surprise; and as some can be shown to be accidental, their occurrence is damaging rather than helpful to the supernatural theory. But even apart from this theory, there is a peculiar fascination to some in this wonderful structure (which very likely has within it more than one yet unexplored chamber). Its latitude, only a mile and one-third south of 30° , and the remarkably correct orientation of its base, both of which must have been fixed by accurate star observations,—the smaller unit of length employed, the 500,000,000 part of the earth's diameter, while the chief unit of length seems to have been the sacred cubit of the 20,000,000 part of that diameter, these probably obtained by very careful measurement of a degree of latitude,—the side of the base having just so many cubits as there are days in the year—the height bearing to the perimeter of the base the ratio of the radius of a circle to the circumference, with fair precision,—these features, which are recognised also by Mr. Proctor, indicate a considerable amount of science for so remote an era as B.C. 2160 or thereabouts. The author's view, on which however he lays no very great stress, though it is ingeniously supported, is that King Cheops was induced by Chaldean shepherd-astronomers to erect the pyramid for astrological purposes. In the last paper, the date 2170 B.C. is assigned to the origin of the constellation figures, and the author thinks that some place between 35° and 39° north was the station of the first founders of solar zodiacal astronomy. In this and some of the other articles, there is of course room for diversity of judgment. For ourselves, whilst not able always to agree with the author's conclusions, we have been much pleased with the work as a whole, which evinces extensive and well-sifted reading. A gentle hint we may perhaps be forgiven in respect of (as seemed to us at least) a certain approach to levity (most likely unconscious) in connection with allusions to passages from the sacred scriptures.

Inaugurazione dell' Osservatorio nel Seminario vescovile di Fiesole: relazione pubblicata per cura della Sezione Fiorentina del Club Alpino Italiano.
In Firenze. 1878.

A collection of addresses given at the inauguration of the new meteorological observatory of Fiesole, published by the Italian Alpine Club, with details of the building and instruments. Monr. Corsani, bishop of Fiesole, Prof. Canonico Nardi, Padre Cecchi, of the Scuole Pie, the Cav. Budden, president of the Florentine section of the Alpine Club, and many others, have contributed to the establishment of this observatory. The addresses by the Canon. Nardi and P. Cecchi on the use, state, and prospects of meteorology are of interest. We learn that the number of meteorological stations in Italy, Alpine, Apennine, and others, is now above 100, a large

number of which have been originated by the Alpine Club (founded 1863). The president mentioned in his address that the greater part of the directors of these are of the clergy. Gifts of books on the natural sciences, &c., will be very acceptable for the library of this new station. They might be addressed to the President of the Florence Section of the Alpine Club, or to the Prof. Canon. Nardi, Director of the Observatory at the Episcopal Seminary of Fiesole.

How to work with the Spectroscope. A manual of practical manipulation with spectroscopes of all kinds, &c., and accessory apparatus. With above thirty engravings and diagrams. By John Browning, F.R.A.S., F.R.M.S., M.R.I., &c. John Browning, 63, Strand, London, W.C. Price, 1s. 6d.

Numerous inquiries respecting the best method of manipulating with various kinds of spectroscopes at last induced Mr. Browning to publish this little work, to which is appended a list of prices. We doubt not that it has been found and will continue to be most acceptable to those who in some form or other use this wonderful instrument.

Southern stellar objects for small telescopes, between the equator and 55° south declination, with observations made in the Punjab. By J. E. Gore, M.R.I.A., A.I.C.E., Assistant-Engineer, Public Works, India. Lodianna. 1877. pp. 114.

The objects mentioned in these pages have been chiefly selected from Sir John Herschel's *Results of astronomical observations at the Cape of Good Hope*, and are intended for telescopes from 2 to 4 inches aperture; and it is hoped that the list will be found useful to amateur observers in south of England latitudes. The book we trust will tend to diffuse a taste for telescopic work in India, where many have fine opportunities of exploring parts of the heavens inaccessible to us here. It will very likely be improved and enlarged if, as we hope, future editions appear. We have observed some misprints, and on pp. 16, 18, "cross" should be substituted for "crucifix."

A theory of Gravity and of the solar process. By Alfred Dawson, F.R.A.S. Basil Montagu Pickering, 196, Piccadilly, London. 1877. pp. 11.

The author argues on the hypothesis of the existence of a magnetic fluid, and says that this essay is the result of a deal of thought, scattered over many years and many subjects. We have not been able to comprehend his theory, and must therefore refer our readers to the pamphlet itself.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE NEW CRATER HYGINUS N.

Sir,—On page 211 of your last number, Mr. W. R. Birt remarks that it is uncertain whether a dark grey marking, seen by Messrs. Sadler, Knott, and Neison during May, 1878, was really the crater which is

supposed to be new by Dr. Klein, or was really a large elliptical depression described by the Rev. R. S. Hutchings, as seen by him to the north of Hyginus. As far as my own observations are concerned there can be no doubt that it was the crater described by Dr. Klein which was observed, and I believe it was this same mark which was seen by Messrs. Knott and Sadler. The elliptical dark grey spot seen by myself was very much smaller than the depression described by Mr. Hutchings. I think the depression observed by Mr. Hutchings must be the elliptical valley which is marked on Map II. of my "Moon" between Hyginus and Hyginus ν (nn). If this be so the valley lies to the north-east of the new crater, and is not really a depression, but merely one of the several shallow valleys enclosed by the numerous ridges in this region. The floor of this valley is considerably lighter than the dark spot Hyginus N., but darker than the plain to the south and east. I do not quite understand from Mr. Birt's remarks whether Mr. Hutchings saw Dr. Klein's crater as such, or merely saw a dark spot in the position of the crater. The dark spot is about five miles by three, and at the southern extremity is bordered by a small white spot. According to Dr. Klein this white spot is really a craterlet. As far as I know it has not been previously observed, and from its position it may be new. It is so small, however, that it could easily have escaped detection.

Whilst writing on this subject, I take the opportunity of referring to the strange article on the new crater in the *Contemporary Review* for this month. Written by an astronomer of great eminence, an author to be admired for the general accuracy and extent of his information, yet it is an article of the most mischievous type, and calculated to give an entirely false impression of the truth. Founded on imperfect, and even erroneous information, it gives such an inaccurate idea of the real matter, that it is impossible to allow it to remain without comment, for an immense amount of mischief would otherwise be done by such an article, coming as it does from the pen of so distinguished an author. Unfortunately, given an unfamiliar subject, imperfect information, and a popular author, and with the best possible intentions you have all the material for unconsciously misinterpreting the truth with a success that ten years labour may not rectify.

The information on which the author has based his article appears to have been an article in *Nature*, for the new crater is said to have been observed on 27th May, 1877, instead of the 19th May, 1877.* It is illustrated by a sort of map, showing the position of the new crater, but I cannot identify the source from which it is taken.

After giving an account of the earlier observations of the new crater, based like all the other published descriptions on the account in the *Selenographical Journal*, the author makes the following remarks:—

"Now, the knowledge that an object has not been seen, may, under certain conditions, amount almost to a moral certainty that it did not exist; and what we have to determine, in the present instance, is the weight of argument from probability, based on the failure of Schröter, Lohrmann, Beer, Mädler, Schmidt, and others, to recognise the crater during their multitudinous observations of the moon. It will be understood, of course, that we have not the direct evidence of any one of these

* The author calculates that on the 27th May, 1877, the moon "had passed its third quarter by rather more than half a day," a capital illustration of the ease with which mistakes can be made in matters so simple that a mistake seems ridiculous. On May 27th, 1877, the moon was really a little past full. Moreover, if it really had been a little past its third quarter, the new crater being situated in the first quarter must have been in the dark part of the moon, and necessarily invisible. Dr. Klein really saw the crater on May 19th, when the moon was a little before its first quarter.

observers in favour of the non-existence of the crater before the year 1877. Not one of them has recorded that, having carefully searched the region enclosed between the two branches of the great rift through Hyginus, they ascertained that there was no crater exceeding—let us say—half a mile (smaller craters not being recognisable with the telescopes they used). If they had, the negative evidence would be as satisfactory and decisive as the best positive evidence. All we know is, that in none of their observations did they take notice of a crater in that particular position.”—Page 45. The conclusion arrived at in these remarks is founded on a misconception. Schröter, Lohrmann, Mädler, and Schmidt all examined this region with the express view of detecting such formations as this crater, and we know that though they saw some smaller craters in its immediate neighbourhood, they did not see this much larger crater. But we know more, we know that at a later period with the powerful Dorpat telescope Mädler carefully re-examined this particular region to see if he could detect any additional features not shown in his map. He found several smaller craterlets in other parts, but he could not detect any other crater in the region where Dr. Klein now states there exists a large crater, though he did find some very small hills close to this spot. This evidence is really conclusive, for it is incredible that Mädler could have seen these minute hills and overlooked a crater so large, that it is the second largest crater of the score in this region. There is, however, still more direct evidence. Dr. Klein describes the new crater as being about three miles in diameter and deep. Dr. Klein has carefully observed this region for twelve years, and is certain that it did not exist previous to 1877. During the six years, 1870—1876, I most carefully examined this region, for the express purpose of detecting any craters not shown by Mädler. I have mapped the region on a larger scale than had yet been done, and I can certify that there did not exist in this region a crater of the character and size specified by Dr. Klein. I repeat, of the character and size of that described by Dr. Klein. A minute craterlet, or a small shallow depression, or a faint dark spot, it is possible might have escaped my observation. A deep black crater, nearly as large as Hyginus, could not have done so.

The author proceeds next to ascertain whether any of the photographs of the moon show this new crater. There does not exist a photograph which will show such objects when near the terminator. It is necessary, therefore, to examine photographs taken when the sun has attained a considerable altitude above this region. On Rutherford's photograph of the third quarter accompanying Proctor's moon, taken on Sept. 16, 1870, he finds a “small dark spot almost exactly where Klein's crater should be.” In photographs taken on March 6, 1865, Feb. 28, 1871, and one or two others there is to be seen a white spot. This he identifies as probably Klein's crater. He then remarks:—

“It is hardly necessary to point out, perhaps, that the recognition of a decided difference of colour in this part of the so-called Sea of Vapours, where none of the lunar map makers have indicated any peculiarity of the kind, tends to throw some degree of doubt on the negative evidence which can alone be adduced in favour of a change of the sort. If the light tinted spot which has certainly existed since 1865, and doubtless, for ages, escaped the scrutiny of lunarians, so also might a dark spot such as is seen when the crater is under low solar illumination. The light tinted spot is not a mere photographic feature—to distinguish thus a peculiarity of tint which shows in a photograph, yet escapes ordinary visual observation. The feature still exists. It can be recognised when the moon is two days old, though it is not so obvious to the eye as in the

photograph. I find, however, that even those observers whose attention has been specially directed to the supposed new crater have failed to recognise the light spot which comes into view a day or so after the crater has assumed the same tint as the surrounding plain. Then it seems quite possible that the crater may have been always in its present condition" (1) (Page 49.)

This conclusion, however, is based on an entire misconception. It assumes, first, that the compilers of the lunar maps pretend to show every variation of tint which they may have seen on the lunar surface, however minute its dimensions. This is entirely incorrect. Mädler, for example, expressly states that his map merely gives the variation of tint when they extend over a considerable area. All minor variations he was obliged to omit. Of course, therefore, his map does not show this white spot as such. Lohrmann and Schmidt follow the same course, which is indeed the only one practical, for, as Mädler has expressly pointed out, these minor variations in tint are variable, and to show them all you would require a dozen maps, each representing the moon under a different stage of illumination. The only other map, my own, does not profess to show differences in tint. It is then assumed that modern observers have not seen this white spot, and so might not have seen the crater itself at an earlier epoch. This is a still greater misconception. I presume it is supposed that they did not see this white spot, because they have not mentioned it in the reports of what they have seen. But there is a very simple explanation of this. I am acquainted, I think, with every published observation of this region made since the existence of the new crater was announced. The observations were made when the crater was to be seen as a dark spot, and as they saw the dark spot which was the new crater, they had no need to trouble themselves about a white spot which could have nothing whatever to do with the new crater, which was the dark spot they did see. But it is an entire fallacy to suppose because they did not mention a white spot they did not see one. On the 11th May, I saw no less than seven white spots immediately around the dark crater, and under favourable conditions at full moon I have seen more than two score of white spots in this region. Which of these is this particular white spot which the author assumes I did not see? As a strict matter of fact, in this region nearly every irregularity in the surface appears like a white spot when under high illumination in good definition. Immediately around the place of the new crater are half-a-dozen smaller craters, all well known, and all showing as white spots. Any one of these might be mistaken on the photograph for the new crater. So far as I know, no one has looked for the new crater under high illumination, for anything which might be seen then would throw no light on the subject. To prove it to be new it must be seen near the terminator as a deep crater two or three miles in diameter.

I have no doubt that the author has mistaken the white spot, principally due to one of these other craters for the crater of Dr. Klein's, which is many miles further north, and which never does appear as a white spot. He has simply mistaken its place. Having carefully examined all the photographs in my possession, including, I think, all those specifically mentioned in the *Contemporary*, besides several others, I cannot find a single white spot in the true position of the crater. There are two white spots near where the crater is supposed to be in the above article, but none in the position where the crater really is. (The whole of this region is slightly lighter than lower-lying regions to the north.) A good telescope under favourable conditions, will show several white spots close to the position of the crater. The photographs fail to show these. The white

spot which the photographs show, and which agrees fairly well with his description, is produced by a small brilliant craterlet, and some neighbouring hillocks. These, together with many others, have been long known to selenographers, and would have misled no one familiar with the subject. All these photographs were examined and the different objects in them in this region identified within 48 hours of the time when Dr. Klein's letter reached England.

With regard to Dr. Klein's crater, I need say no more. It has not yet been proved to be new. Ever since April the weather has been so persistently bad in England, when the crater has been well placed for examination, that no satisfactory observation has been made. One suitable evening will settle the matter. If I find a deep black crater three miles in diameter, in the place assigned to it by Dr. Klein, and when six years' observation convinces me no such crater did exist, I shall know that it must be new.

I may as well mention two other misconceptions in the article, as they are of some importance. Thus, referring to the case of Linné, it is stated on page 41, that, "It is noteworthy, however, that among Schröter's observations of Linné is one recording that in November, 1788, the crater was occupied by a dark spot, instead of appearing as usual somewhat brighter than the neighbouring regions." This is, however, a mistake. Schröter only observed Linné *once*, namely, on Nov. 5, 1788, and he then saw what is commonly thought to have been Linné as a white spot, and not as a dark spot. The only dark object he saw was an indistinct depression presenting the same appearance as the deep crater Bessel *m*. The objects were near the terminator and the definition bad. This is a very different thing to what the article would lead you to suppose. The other point is with regard to the dimensions of Linné. We are asked, "Are we, then, to believe that a crater seven miles across and two or three miles deep (nothing less would correspond with Lohrmann's and Mädler's description of 'very deep') had become in some way filled up to within a third of a mile of its lips?" (page 41.) This is a ridiculous interpretation of the description of "very deep," stated to be given by Lohrmann and Mädler to the crater. By the way, Mädler never did describe it as "very deep." There is not a crater on the moon of anything like the diameter of Linné (which lip really had an aperture of only four to five miles, as Mädler's measure applied to its outer diameter) which is known to have any such depth.

In fact it is very doubtful if there are two or three ring plains on the moon which have a depth of three miles beneath the crest of their walls, and these are fifty miles in diameter, not five. But though Lohrmann and Mädler have described many craters of the size of Linné or even twice this size, as being "deep," "very deep," or "remarkably deep," or even of "most extraordinary depth," there is scarcely one which exceeds a mile and a half in depth, and the great majority are less than a mile in depth.* The present depth of the shallow crater of Linné, if there be a shallow crater, so far from being one-third of a mile it does not exceed one-tenth of this. Interpreting Lohrmann's and Mädler's description of Linné as it reasonably may be interpreted, the depth of Linné may be taken as from one-half to two-thirds of a mile. Schmidt says one-fifth of a mile.

E. NEISON.

August 13th, 1878.

* The very next crater to Linné which is described by Lohrmann as being "very deep," is Archimedes A, a crater nearly eight miles in diameter, and just 3,000 feet deep, little more than half a mile.

JUPITER ON JULY 31, 1878.

Sir,—I should be glad to know if any observer, looking at Jupiter from about 10 p.m. to 11 p.m. on the 31st ult., noticed some remarkable movements of light and cloudy matter between the belts. The weather here was not at all favorable to exact vision. The wind was N.E. and cold, and a light mist driving about rapidly caused sudden and rather violent changes in the luminosity of the disk; for a few seconds it was very brilliant, then dull, and so on. Under these difficulties no very definite conclusion was possible, but what appeared to be going on was a considerable change in the position of the most luminous spots between the belts. At 10 o'clock there was a bright patch of a roundish form a little to the left of the centre of the disk, and two ill-defined patches less bright on either side of it, the interspaces being a shade or two darker. By 10.45 p.m. the brightest spot was considerably to the left of its position half-an-hour earlier, and the general configuration of the light and darker spaces between the belts had altered.

The unsteadiness of the air and the action of the floating mist could not, I think, have accounted for the whole of these appearances, but they entirely prevented steady continuous vision. The power used was one of Browning's achromatics $\times 181$, and the telescope the With-Browning reflector of $6\frac{1}{2}$ aperture.

Ashdown Cottage, Forest Row,
Sussex: Aug. 3, 1878.

I remain, &c.,

HENRY J. SLACK.

MINOR PLANET PALLAS.

The minor planet Pallas, the second in the series, comes into opposition on the 10th of the month. The following is its position over the meridian of Greenwich:

	R.A.			Decl.		
	h.	m.	s.	N.	°	'
Sept. 2	23	12	59	N.	1	26 13.1
8	23	8	30	N.	0	8 47.4
10	23	6	59	S.	0	18 03

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN
SEPTEMBER, 1878.

By W. R. BIRT, F.R.A.S., P.S.S.

KLEIN'S NEW CRATER. In our last list, see *ante* p. 210, we called attention to this object, soliciting observations of it that might have been obtained in July, June, or earlier. Miss Ashley, of Bath, has sent us a record of observations made on July 14, 19 and 20, accompanied with sketches. That of July 14 shows a dark imperfect oval in the position of Mr. Hutching's oval, with four white spots on the border. South-west of this oval Miss Ashley gives a round dark spot, which she took for Klein's new crater. This dark spot appears to be identical with Hutching's No. 6 in his drawing (see *English Mechanic*, July 5, p. 407). She says, "The object I considered to be the new crater I judged to be so from its position with regard to the spiral mountains and Hyginus, which seemed to be exactly the position given in Dr. Klein's drawing in the *English Mechanic*, April 5, p. 92. I saw it with great difficulty; it was a dark marking, about half the size of Hyginus. I should not have taken it for

a crater. On July 20th, 14h. 30m., terminator a little to the west of Godin and Agrippa, I saw the spiral complete. At first I could not see the new crater, but a little later, at 15h., I thought I could distinguish it as a dark mark much smaller than Hyginus, in the same position as I saw it the night before [the 19^o and in sketch of July 14, W. R. B.]. These are the notes I made at the time."

Miss Ashley gives in her sketch of July 14, Mr. Hutching's craters 6 and 2 in his drawing (see *English Mechanic*, July 5), a line drawn through both craters in both drawings passes through Miss Ashley's and Mr. Hutching's elliptical depressions, but not similarly. Miss Ashley has two additional craters N.W. and S.E. of Hutching's No. 2 and in a line with it. Miss Ashley has also a crater south of and close to Hyginus, which she describes as of the same shade as the interior of the oval. Careful drawings of the region will be very important.

Stratford, E. 1. August 20, 1878.

THE PLANETS FOR SEPTEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	11 33 47	S. 1 56	10".1	0 51.6
	9th	{ 11 11 8 } { 11 7 47 }	{ N. 1 17 } { N. 1 53 }	10".5	23 50.3
	17th	10 51 15	N. 6 15½	8".9	23 2.3
	25th	11 7 53	N. 6 48	7".0	22 47.5
Venus ...	1st	9 14 26	N. 16 44½	11".0	22 28.7
	9th	9 53 21	N. 13 54	10".8	22 36.1
	17th	10 31 23	N. 10 36	10".6	22 42.5
	25th	11 8 43	N. 6 59½	10".4	22 48.3
Jupiter ...	1st	20 1 18	S. 21 14½	43".1	6 17.7
	9th	19 59 25	S. 21 20	42".1	8 44.4
	17th	19 58 22	S. 21 22½	41".3	8 11.9
	25th	19 58 10	S. 21 23	40".3	7 40.2
Saturn ...	1st	0 7 46	S. 1 54½	17".4	13 23.5
	9th	0 5 41	S. 2 9	17".6	12 50.0
	17th	0 2 55	S. 2 28	17".6	12 16.3
	25th	0 1 13	S. 2 39	17".6	11 42.6
Neptune ...	14th	2 30 53	N. 12 57	...	14 55.1
	30th	2 29 39	N. 12 50	...	13 51.0

Mercury sets nearly an hour after the sun at the beginning of the month, the interval decreasing. Towards the middle of the month he rises before the sun, the interval increasing to about an hour and three quarters on the last day.

Venus rises about two hours and a quarter before the sun on the 1st, the interval decreasing.

Jupiter sets an hour and a half after midnight at the beginning of the month, the interval decreasing. Towards the end of the month he sets before midnight.

Saturn rises three-quarters of an hour after sunset, at the beginning of the month, the interval decreasing.

ASTRONOMICAL OCCURRENCES FOR SEPTEMBER, 1878.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage
		h. m.			h. m. s.	h. m.
Sun	1			4th Oc. R. 4th Ec. D. 1st Oc. D.	7 13 10 53 34 11 3	Jupiter. 9 17.7
Mon	2		Saturn's Ring: Major axis = $43^{\circ} 63'$ Minor axis = $2^{\circ} 90'$	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	8 11 9 5 10 31 11 25	9 13.5
Tues	3	8 25	Moon's First Quarter	1st Ec. R. 3rd Oc. R. 3rd Ec. D. 3rd Ec. R.	8 43 16 9 8 9 18 4 12 46 33	9 9.3
Wed	4			2nd Tr. I. 2nd Sh. I. 2nd Tr. E.	10 3 11 55 12 59	9 5.1
Thur	5	10 47	Occultation of ϕ Sagittarii 3 $\frac{1}{2}$			9 1.0
		7 31	Occultation of κ^1 Sagittarii (6)			
Fri	6	8 44 21 23	Reappearance of ditto Conjunction of Mars and Mercury $5^{\circ} 27' S.$ Conjunction of Moon and Jupiter $0^{\circ} 43' N.$	2nd Ec. R.	8 58 39	8 56.8
Sat	7					8 52.7
Sun	8			1st Oc. D.	12 52	8 48.5
Mon	9	12 2 19	Near approach of B.A.C. 7697 (6 $\frac{1}{2}$) Inferior conjunction of Mercury and Sun	1st Tr. I. 1st Sh. I. 4th Tr. I. 1st Tr. E.	9 59 11 0 11 33 12 19	8 44.4
Tues	10	14 8 14 39	Occultation of B.A.C. 7986 (6)	1st Oc. D. 3rd Oc. D. 1st Ec. R. 3rd Oc. R.	7 19 9 4 10 38 33 12 40	8 40.3
Wed	11	3 49	O Full Moon	1st Tr. E. 1st Sh. E. 2nd Tr. I.	6 46 7 49 12 26	8 36.2
Thur	12	9	Conjunction of Moon and Saturn $7^{\circ} 6' S.$			8 32.1
Fri	13			2nd Oc. D. 2nd Ec. R.	6 32 11 35 11	8 28.0
Sat	14			3rd Sh. E.	6 52	8 24.4
Sun	15	10 49	Near approach of 26 Arietis (6) Illuminated portion of disc of Venus = 0.936 Illuminated portion of disc of Mars = 1.000	2nd Sh. E.	6 47	8 19.9

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.		1st Tr. I.	h. m. s.	h. m.
Mon	16				11 48	Jupiter. 8 15.9
Tues	17	15 39	Near approach of χ^1 Tauri ($5\frac{1}{2}$)	1st Oc. D.	9 8	8 11.9
Wed	18	0	Conjunction of Mars and Sun	1st Tr. I. 1st Sh. I. 1st Tr. E. 4th Oc. R. 1st Sh. E.	6 16 7 24 8 36 9 34.49 9 44	8 7.9
Thur	19	6 30	☾ Moon's Last Quarter	1st Ec. R.	7 2 45	8 3.9
Fri	20	10 43 14 30 15 7	Near approach of ω Geminorum (6) Occultation of 48 Geminorum (6) Reappearance of ditto	2nd Oc. D.	8 58	7 59.9
Sat	21			3rd Tr. E. 3rd Sh. I. 3rd Sh. E.	6 8 7 15 10 54	7 55.9
Sun	22	4	Saturn at opposition to the Sun Saturn's Ring : Major axis = $43^{\circ}95'$ Minor axis = $2^{\circ}44'$	2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	6 28 7 2 9 24	7 52.0
Mon	23					7 48.1
Tues	24	22 23 23	Conjunction of Venus and Mercury $0^{\circ}28' S.$ Conjunction of Moon and Mercury $4^{\circ}7' N.$ Conjunction of Moon and Venus $4^{\circ}34' N.$	1st Oc. D.	10 59	7 44.1
Wed	25			1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	8 7 9 19 10 27 11 40	7 40.2
Thur	26	2 10 1	● New Moon Conjunction of Moon and Mars $5^{\circ}16' N.$	4th Tr. E. 1st Ec. R.	8 25 8 58 7	7 36.3
Fri	27			1st Sh. E. 2nd Oc. D.	6 9 11 26	7 32.5
Sat	28			3rd Tr. I. 3rd Tr. E. 3rd Sh. I.	6 16 9 52 11 16	7 28.6
Sun	29			2nd Tr. I. 2nd Sh. I. 2nd Tr. E.	6 34 9 5 9 30	7 24.7
Mon	30	2	Conjunction of Venus and Mercury $0^{\circ}34' N.$			7 20.9
OC T. Tues	1			2nd Ec. R.	6 7 41	7 17.1

JUPITER.

1878. oh. Gr.	Angle of position of J's axis.	Latitude of Earth Sun above J's equator.		Equat. diameter.	Defect of illumi- nation.
Sept. 2	346°73	—0°64	—0°51	45°49	0°20
12	346°93	0°65	0°46	44°33	0°27
22	347°01	—0°65	—0°42	43°08	0°33

The assumed First Meridian passes the middle of the illuminated disk at the following times :

1878. G. M. T.

	h. m.		h. m.		h. m.
Sept. 1	6 29.2	Sept. 11	4 40.2	Sept. 21	2 51.9
	16 24.6		14 35.7		12 47.4
2	2 20.1	12	0 31.2		22 42.9
	12 15.6		10 26.7	22	8 38.4
	22 11.0		20 22.1		18 33.9
3	8 6.5	13	6 17.6	23	4 29.4
	18 11.9		16 13.1		14 24.9
4	3 57.4	14	2 8.6	24	0 20.4
	13 52.8		12 4.1		10 15.9
	23 48.3		21 59.5		20 11.4
5	9 43.7	15	7 55.0	25	6 6.9
	19 39.2		17 50.5		16 2.4
6	5 34.6	16	3 46.0	26	1 58.0
	15 30.1		13 41.5		11 53.5
7	1 25.6		23 37.0		21 49.0
	11 21.0	17	9 32.5	27	7 44.5
	21 16.5		19 28.0		17 40.0
8	7 12.0	18	5 23.4	28	3 35.5
	17 7.4		15 18.9		13 31.0
9	3 2.9	19	1 14.4		23 26.6
	12 58.4		11 9.9	29	9 12.1
	22 53.8		21 5.4		19 17.6
10	8 49.3	20	7 0.9	30	5 13.1
	18 44.8		16 56.4		15 8.6
				Oct. 1	1 4.2
					10 59.7

A. MARTH.

THE INNER SATELLITES OF SATURN.

Major and minor semi-axes of the apparent orbits of the inner satellites, expressed in semi-diameters of Saturn's equator.

		Mimas.	Ence.	Tethys.	Dione.	Rhea.
Major		3.14	4.03	4.99	6.40	8.93
Minor	Sept. 2.	0.21	0.27	0.34	0.43	0.60
	12.	.19	.25	.31	.39	.55
	22.	.17	.22	.28	.35	.49
	Oct. 2.	0.15	0.20	0.24	0.31	0.44

Approximate Greenwich times of some of the conjunctions of the satellites with the ends of the ring, and also of some of their greatest elongations.—V. p. 189 :

1878.	Gr.	M.	Time.	
			h.	
Sept. 1	11.6	Rhea.	w.	
	12.0	Dione.	sf.	
	12.3	Encel.	sp.	
	13.9	Tethys.	e.	
2	10.0	Rhea.	sp.	
	11.3	Encel.	np.	
	12.6	Tethys.	w.	
	13.0	Dione.	nf.	
	14.6	Mimas.	w.	
3	8.6	Encel.	e.	
	9.3	Dione.	w.	
	11.2	Tethys.	e.	
	13.2	Mimas.	w.	
	13.6	Encel.	nf.	
4	9.8	Tethys.	w.	
	11.8	Mimas.	w.	
	12.6	Encel.	sf.	
	16.2	Rhea.	nf.	
5	8.5	Tethys.	e.	
	10.5	Encel.	w.	
	10.5	Mimas.	w.	
	14.5	Dione.	np.	
	14.9	Encel.	sp.	
	16.3	Tethys.	nf.	
6	9.1	Mimas.	w.	
	13.9	Encel.	np.	
	14.9	Tethys.	sp.	
	15.4	Dione.	sp.	
7	7.6	Rhea.	sf.	
	11.3	Encel.	e.	
	11.8	Dione.	e.	
	13.6	Tethys.	nf.	
	16.2	Encel.	nf.	
8	8.2	Dione.	np.	
	8.7	Encel.	sp.	
	12.2	Tethys.	sp.	
	15.2	Encel.	sf.	
9	9.1	Dione.	sp.	
	10.8	Tethys.	nf.	
	12.6	Encel.	w.	
	13.8	Rhea.	np.	
10	9.5	Tethys.	sp.	
	10.0	Encel.	nf.	
	12.2	Rhea.	w.	
	14.8	Mimas.	e.	
11	8.1	Tethys.	nf.	
	9.0	Encel.	sf.	
	10.7	Rhea.	sp.	
	13.5	Mimas.	e.	
	13.9	Encel.	e.	
	14.3	Dione.	w.	
	15.2	Tethys.	np.	
12	10.7	Dione.	sf.	
	11.3	Encel.	sp.	

1878.	Gr.	M.	Time.	
			h.	
Sept. 12	12.1	Mimas.	e.	
	13.9	Tethys.	sf.	
13	10.3	Encel.	np.	
	10.7	Mimas.	e.	
	11.6	Dione.	nf.	
	12.5	Tethys.	np.	
	15.2	Encel.	w.	
	16.8	Rhea.	nf.	
14	7.9	Dione.	w.	
	9.3	Mimas.	e.	
	11.2	Tethys.	sf.	
	12.6	Encel.	nf.	
15	9.8	Tethys.	np.	
	11.6	Encel.	sf.	
16	8.3	Rhea.	sf.	
	8.5	Tethys.	sf.	
	9.0	Encel.	w.	
	13.1	Dione.	np.	
16	13.9	Encel.	sp.	
	16.2	Tethys.	e.	
17	6.7	Rhea.	e.	
	7.1	Tethys.	np.	
	12.9	Encel.	np.	
	14.0	Dione.	sp.	
	14.9	Tethys.	w.	
18	10.3	Encel.	e.	
	10.4	Dione.	e.	
	13.5	Tethys.	e.	
	14.4	Rhea.	np.	
	15.1	Mimas.	w.	
	15.2	Encel.	nf.	
19	12.2	Tethys.	w.	
	12.9	Rhea.	w.	
	13.7	Mimas.	w.	
	14.2	Encel.	sf.	
20	10.8	Tethys.	e.	
	11.3	Rhea.	sp.	
	11.6	Encel.	w.	
	12.3	Mimas.	w.	
	15.6	Dione.	sf.	
21	9.0	Encel.	nf.	
	9.5	Tethys.	w.	
	10.9	Mimas.	w.	
	15.5	Encel.	np.	
22	8.0	Encel.	sf.	
	8.1	Tethys.	e.	
	9.5	Mimas.	w.	
	12.9	Dione.	w.	
	12.9	Encel.	e.	
	15.9	Tethys.	nf.	
23	9.3	Dione.	sf.	
	10.3	Encel.	sp.	
	14.5	Tethys.	sp.	
24	9.3	Encel.	np.	

1878. Gr. M. Time.

	h.	
Sept. 24	13.2	Tethys. nf.
	14.2	Encel. w.
25	8.9	Rhea. sf.
	11.6	Encel. nf.
	11.8	Tethys. sp.
26	7.2	Rhea. e.
	10.5	Tethys. nf.
	10.6	Encel. sf.
	15.3	Mimas. e.
	15.4	Dione. e.
	15.5	Encel. e.
27	8.0	Encel. w.
	9.1	Tethys. sp.
	11.7	Dione. np.
	12.9	Encel. sp.
	13.9	Mimas. e.
	15.1	Rhea. np.
28	7.8	Tethys. nf.

1878. Gr. M. Time.

	h.	
Sept. 28	11.9	Encel. np.
	12.5	Mimas. e.
	12.7	Dione. sp.
	13.5	Rhea. w.
	14.9	Tethys. np.
29	9.0	Dione. e.
	9.3	Encel. e.
	11.1	Mimas. e.
	12.0	Rhea. sp.
	13.5	Tethys. sf.
	14.2	Encel. nf.
30	9.8	Mimas. e.
	12.2	Tethys. np.
	13.2	Encel. sf.
Oct. 1	10.6	Encel. w.
	10.8	Tethys. sf.
	14.2	Dione. sf.

The position angles of the axes of the apparent orbit of Iapetus are on Sept. 2, $12^{\circ}57'$, and on Oct. 2, $12^{\circ}36'$ less than those of the inner satellites, the minor semi-axis being on Sept. 2, $9^{\circ}36'$, and on Oct. 2, $8^{\circ}72'$, and the major semi-axis $60^{\circ}34'$ semi-diameters of Saturn's equator, Iapetus is near its superior conjunction, south of the planet, on Sept. 2 and near its greatest eastern elongations on Sept. 21. A. M.

Books Received.—Report of the New Jersey State Commission for Encouragement of Manufactures. Trenton, N. J.: Naar, Day, & Naar. 1878.—Instructions for Observing the Total Solar Eclipse. Washington: Government Printing Office. 1878.—Annals of the Astronomical Observatory of Harvard College. Vol. IX. Leipzig: Wilhelm Engelmann. 1878.—Report of the Kew Committee for the year ending Oct. 31, 1877.—Elements of Sidereal Astronomy. By Jacob Ennis. Philadelphia: Collins. 1878.—Rathmines School Magazine. Dublin. 1878.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.
To June, 1878.

Knobel, E. B.

To Sept., 1878.

Jones, W. E.

To Oct., 1878.

Benson, Rev. Dr.

To June, 1879.

Elger, T. G. E.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for the Editor are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The Astronomical Register.

No. 190.

OCTOBER.

1878.

REMARKS ON THE OBSERVATIONS OF THE ECLIPSE OF JULY 29th, 1878.

BY MR. RANTARD.

On my return to England, after observing the recent eclipse, I was somewhat surprised to find that the reports which had appeared in the English newspapers had described the corona as very much fainter than the coronas observed during the eclipses of 1870 and 1871. Judging from my remembrance of the corona of 1870, which I observed with a fairly clear sky, at Villasmunda in Sicily, I should have said that the corona of this year was decidedly brighter than that of 1870; and I believe that Prof. Young and Mr. Penrose, both of whom observed the eclipse of 1870 in Spain, are of the same opinion; the photographs also seem to tell the same tale: at all events as to the lower regions of the corona, say up to the height of 15' above the sun's limb.

Mr. Brothers' Syracuse photograph shows a greater extension of the corona than any of the photographs which have been taken during the recent eclipse, but there is ample evidence to show that the height of the corona in the equatorial regions, as seen by the naked eye,* was much greater during the recent eclipse than in 1870. Newcomb, Langley, and Abbe, all three, I believe, estimated that the corona extended to a distance of twelve solar diameters in the region of the solar equator, and I

* It seems, therefore, that the corona of 1878 was brighter in its lower regions and again in the higher parts of the equatorial regions, while the corona of 1870 was, probably, brighter in an intermediate region to the north and south, as well as to the east and west of the sun. Judging by Mr. Brothers' photograph, the intermediate region of greater brightness extended from a height of 15' or 20' up to a height of about 45' above the limb.

have seen numerous drawings giving an extension of two and three solar diameters to the corona in the equatorial regions, whereas I know of no such drawings made during the eclipses of 1870 and 1871.

As to the relative brightness of the lower parts of the corona in 1871 and 1878, I am inclined, from an examination of the photographic evidence, to conclude that up to a height of 20' or 25' the corona of 1871 did not differ very greatly in brightness from that of 1878. There is a considerable element of uncertainty in estimating the brightness of objects photographed from the density of the photographic action, but I think we may feel sure that the corona of 1871 was not ten times as bright as that of 1878, as I see that Mr. Lockyer reported. Indeed it seems to be rather more probable that the corona of 1878 was a little the brighter of the two. The most striking part of the photographic evidence may be thus shortly stated.

The photographs taken by Dr. Brackett during the recent eclipse with a six-inch telescope of rather more than seven feet focal length, seemed, as far as I could judge, to be about equally dense with the photographs of similar exposure of Lord Lindsay's and Col. Tennant's series, taken in India in 1871. I speak with some caution, as I have not had an opportunity of comparing the negatives side by side; but the density of the Indian negatives is very well impressed on my mind, as I spent more than a year in making out and cataloguing the details which are visible upon them. It must be borne in mind that, during the Indian eclipse, the sun, as seen from the two photographic stations, was at a lower altitude than as seen from Denver, Dr. Brackett's station; but while the angular aperture of Dr. Brackett's instrument was less than one in fourteen, Lord Lindsay's and Col. Tennant's cameras were of four inches aperture, and about thirty inches focus, or one in seven-and-a-half—that is, in the two Indian cameras, which were of similar construction, the pencil of light falling on an element of the plate, was nearly four times as intense as the pencil falling on a similar element in Dr. Brackett's telescope camera. We have no data, of course, for comparing the *diactinism* of the Indian and Colorado atmospheres. But we may, I think, feel justified in asserting that as far as the evidence derived from the photographs of the 1870, 1871, and 1876 goes, the corona does not wax and wane with sun-spots. In fact, the corona with by far the greatest equatorial extension, is found at a period when there are hardly any spots upon the sun's disc, and it should also be remembered that the difference between the coronas of 1870 and 1871 does not at all correspond with the development of sun-spots at the two periods.

Secondly, I see that it has been stated that the corona seen during the recent eclipse was remarkable for the absence of bright lines in its spectrum. It is perfectly true that none of the observers who made use of spectroscopes without slits saw the bright rings corresponding to the 1474 and the hydrogen lines which Mr. Lockyer and Prof. Respighi described as having been visible during the Indian eclipse; but Prof. Young, making use of an ordinary spectroscope, that is, one provided with a slit, saw the 1474 line very distinctly, and traced it to a considerable altitude above the sun's limb.

The rings were carefully searched for on the present occasion, with various kinds of instruments. I occupied myself for ten or fifteen seconds after the commencement of totality in gazing at the corona with a direct vision prism held in front of the eye, but I could see no rings upon the band of colour into which the light of the corona was spread out. Prof. Rockwood spent nearly the whole of totality in searching for the monochromatic images with a binocular instrument, with which, if the rings had been visible, at all, they would certainly have been seen. But not a trace of them was to be detected, though Prof. Young, who was observing within twenty yards of Prof. Rockwood, with a grating of 17,000 lines to the inch and a spectroscope with a slit, saw the 1474 line with ease, and traced it to a height of 20' from the sun's limb.

This strikingly illustrates the great difference between a pure and an impure spectrum. The green line in Prof. Young's spectrum was of course no brighter than the green image in Prof. Rockwood's field of view, but in the latter case the green image of the corona was overlapped and drowned out of view by other images corresponding to adjacent wave lengths. The object of a slit is to prevent such a drowning out of bright or dark images. Everyone who has used a spectroscope must be familiar with the fact that narrowing the slit decreases the brightness of the spectrum, while at the same time the visibility of the bright or dark lines is increased. It will consequently be evident that the best conditions for seeing bright lines will not be the best conditions for obtaining a spectrum which will be bright enough to photograph. With a spectroscopic camera of such dispersion that the images of the corona are spread out on the photographic plate to a distance of five or six times the diameter of the corona, it is obvious that the impurity of the spectrum, when no slit is used, will be very great; but the brightness of the band of light—it is hardly proper to call it a spectrum—will be only eight or ten times less than the brightness of an image of the corona thrown without a prism or grating upon the plate. I say eight or ten

times, instead of five or six, in order to make allowance for the light lost by reflection and transmission through the prism or at the grating, as the case may be.

Since the corona could be photographed in a few seconds, it was evident that such a band of light would probably leave its trace upon a photographic plate exposed during the whole of totality. This is what has been accomplished during the recent eclipse; but it is not photographing the continuous spectrum of the corona, as I see that many persons have assumed. The difference between this band of light and the continuous spectrum on which the bright lines of the corona were* believed to be projected, is so obvious that I should not have entered into such an elementary explanation if I had not seen that the editor of an important astronomical journal, in reviewing the results that have been obtained during the recent eclipse, assumes that the continuous spectrum of the corona has really been photographed, and asserts that it would have been easier to photograph the bright lines of the corona, if they had been present, than to photograph such a continuous spectrum. In 1875, when preparations were being made to photograph the spectrum of the corona during a total eclipse which was about to take place in Siam, I published reasons for believing that the attempt was not likely to be successful. Up to that time, and indeed up to the present time, no photograph of the corona has been obtained in a secondary focus. If a pure spectrum is required, a slit (which must be placed in the primary focus) must be made use of, and the photographic plate will consequently have to be placed in a secondary focus. Even supposing that the light of the corona were monochromatic, it is doubtful, when we take into account the loss of light at the prism, or grating, and the decrease of light by reason of the magnified image, whether a photographic record could be obtained with an exposure during the whole period of totality. But it must be remembered that the light of the corona consists of several bright lines, and that many considerations† tend to show that the

* I say "were" believed, because several recent observations, as well as general considerations, derived from polariscopic and other observations, tend to prove that the background is only a faint ordinary solar spectrum, containing the dark Fraunhofer lines.

† It is difficult, in the short space of a note, to do more than indicate these considerations, which have been referred to more fully on former occasions. It is evident that the monochromatic part of the light of the corona, which is due to the light emitted by incandescent gas, will be unpolarized, and polariscopic observations show that a very large proportion of the light of the corona is polarized. If we rely on the polarimeter measures that have been made, and take into account the large angle which the sun must subtend to particles in the lower parts of the corona,

total light of the bright lines forms only a small proportion of the total light of the corona.

With the slit placed in front of the object glass it will be seen that the conditions would be still more unfavourable, and the spectrum, if any were obtained, would be an integration spectrum, in which the light of the prominences would be combined with that of the corona.

Without a slit, the overlapping images greatly increase the difficulty, in order that a monochromatic image should show itself on the photographic plate, it would be necessary that its brightness should be sufficient to give rise to a difference of intensity that would be visible upon a fogged background.

Many people are not aware that the eye can distinguish much fainter lights than can be photographed by any process in present use. I have made prolonged attempts, making use of various precautions which it would take too much space to describe here, to photograph some of the brighter nebulae, but I have not at present succeeded in obtaining even a trace of the Dumb-bell nebula or the brighter parts of the nebula in Orion. I thought, however, that it might be possible to obtain a photographic register of the bright lines seen in the spectrum of the base of the chromosphere at the beginning and end of totality. During the eclipse of 1860, and in more recent eclipses, the prominences have impressed their images on photographic plates in a very short space of time, probably in some small fraction of a second.

it is evident that the unpolarized light can bear only a small proportion to the light dispersed by the polarizing small particles of the corona.

It seems probable that the monochromatic light of the corona is equally distributed through the rifts and brighter structure, and that it bears only a small proportion even to the light of the rifts. Herschel and Tennant, observing in 1871, saw the bright 1474 line extending evenly across the field of view when the slit of the spectroscope was placed across the edge of the great southern rift, so as partly to lie upon a very bright part of the corona, and partly extend into the area of the rift. Respighi, in the rings he observed, saw no indentations corresponding to the rifts. Carpmæl, in 1870, also seems to have placed the slit of his spectroscope across the lower part of one of the rifts, and he speaks of "the bright corona line" as "extended entirely across the field of view, but fainter towards the extremities." These observations, taken in conjunction with the fact that the light of the rifts shows very decided polarization, would tend to show that the monochromatic light bears only a small proportion to the light of the rifts.

That the light of the continuous spectrum or field between the bright lines of the coronal spectrum is considerable cannot be doubted. Denza and Lorenzoni, in 1870, Fyers, in 1871, and Stone, in 1874, speak of the continuous spectrum as showing definite colours. Janssen, in 1871, and Stone, in 1874, and I understand other observers during the recent eclipse, speak with confidence of having observed dark lines in the continuous spectrum seen with a narrow slit.

We know that in the prominences the intensity of the light usually increases very rapidly as you proceed downwards, and I consequently hoped that the bright lines in the spectrum in this part of the chromosphere might be sufficiently luminous to impress their images on the photographic plate in the two or three seconds during which the bright line layer at the base of the chromosphere on Young's stratum, as it is frequently called, usually remains visible at the beginning and end of totality.

For this purpose no slit would be necessary, as the bright line giving stratum at the centre of the disappearing crescent, would itself form a line of light sufficiently narrow to give a very pure spectrum.

In order to make the experiment and at the same time to determine how far the streak of light, formed of the superposed images of the corona, would leave its trace upon the photographic plate, I constructed a camera with a short focussed nearly achromatic lens, which had been lent me by Lord Lindsay. The lens has a focal length of only 8 inches, and a diameter of $3\frac{1}{4}$ inches. I was, however, obliged to reduce its aperture by making use of a diaphragm with an opening of rather more than two inches square. In front of the lens and diaphragm was placed a flint prism of 45° , lent me by Prof. Young. The whole was mounted on the top of my larger camera and was driven by clockwork. Half a minute or perhaps a little more before the commencement of totality I placed myself beside the instrument with a direct vision prism held in front of the eye. The central parts of the crescent gave a bright band across the field, on either side of which were projecting spikes of light corresponding to the monochromatic images of the cusps. The central band became rapidly narrower and the monochromatic images of the cusps became more striking till the moment when I saw the central band, which had been reduced to a breadth of 3' or 4', commence to break up. I then made the exposure, and left the camera so exposed during the whole of totality. It was covered up again just as light was commencing to appear on the other limb. The plate, which was one of "Mawson and Swan's extra sensitive dry plates," shows a diffused band corresponding to some of the superposed images of the lower and more brilliant part of corona; but as far as I can see there are no bright lines from the centre of the disappearing crescent, and there are no traces of the chromospheric images of the cusps which were conspicuously visible at the time I made the exposure. The trace of the coronal images can only be described as faint even at its densest parts, and it fades off rapidly in either direction, showing that the combined action of the over-lapping images

was only sufficient to leave a trace in a restricted region of the spectrum.

Upon the same plate a little below the superposed images of the corona there is an over-exposed spectrum of the thin crescent of the reappearing sun—which printed itself after the camera had been shifted from its original position—at the moment that I covered the prism with its cap—for at the end of totality I had put an end to the exposure by covering the front of the prism with my hand—and afterwards in removing the hand and placing on the cap this exposure seems to have been made. Although this happened by accident, it turns out to be a very convenient scale of reference. At first it misled me and many of my friends, as we took it to correspond to the spectrum of Young's stratum seen at the east and west limbs. This mistake was rendered possible by the fact that the spectrum is much over-exposed, and the central parts in the neighbourhood of the G line are rendered quite transparent by the over-exposure; but the explanation is now clear, and it serves to show that the superposed images of the corona which have imprinted themselves correspond to only about a fourth or a fifth part of the length of the photographed spectrum. It also shows, on the assumption that the shift of the camera took place in declination, that the most intense monochromatic images of the corona are to be found in the neighbourhood of G; a fact which adds weight to the assumption that the light of the corona chiefly consists of dispersed solar light, which also has its maximum of action near G.

THE ETHICS OF ASTRONOMY.

We would say a little on *mores astronomici*, the ethical accompaniments of our favourite science, and their corresponding deportment. *Imprimis*, modesty should be enshrined in the heart of the astronomer. The spectacle opened to him above other men is of surpassing grandeur, and must impress him not only with a sense of his personal insignificance, but of that of the great globe itself which he inhabits, and the further he advances the more he can sympathise with the well-known saying of the immortal Newton regarding himself,* the more he will feel with Laplace the disparity between his knowledge and what there is

* "I know not," he remarked a short time before his death, "what I may appear to the world, but to myself I seem to have been only like a boy playing on the sea-shore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me."

to be known.* In proportion as this sentiment is profound will he be likely to be considerate and courteous towards others. There is, indeed, a polish and high breeding acquired by mixing early and much in what is called good society, which makes a person careful not to inflict even a trifling mortification on another. In this respect D'Alembert and Clairaut present an amusing contrast. The biographer of the latter says, "These two great men were rivals in their scientific labours, and though their disputes never passed the bounds of courtesy, the life of each, with respect to the other, was either armed truce or open war. The characters of the two were essentially opposite; Clairaut was a man of the world, of high polish, and who took great care never to offend the self-love of any one; D'Alembert was blunt and rude, though essentially well meaning and kind—if we may use such a colloquial phrase, he 'stood no nonsense.' 'J'aime mieux être incivil qu'ennuyé,' was his avowed maxim. Clairaut was always in the world, desirous to shine, and to unite the man of fashion with the philosopher, of all which D'Alembert was the reverse. The attacks usually came from the latter, confined entirely to the writings of his opponent; and he was frequently right, being a thinker of a more safe and cautious order than Clairaut, who was more than once too hasty."†

This consideration for others was thus in Clairaut, as is often the case, merely a surface quality. But whilst the student of

* "Ce que nous connaissons est peu de chose, ce que nous ignorons est immense," were, it has been said, the last words of this illustrious astronomer, but Mr. Proctor, in a note to p. 113 of his "Myths and Marvels of Astronomy," shows that these were not his last words. The note is of such interest that we are induced to copy it here. "De Morgan gives the following account of Laplace's last moments, on the authority of Laplace's friend and pupil, the well-known mathematician, Poisson: 'After the publication (in 1825) of the fifth volume of the *Mécanique Céleste*, Laplace became gradually weaker, and with it musing and abstracted. He thought much on the great problems of existence, and often muttered to himself, '*Qu'est ce que c'est que tout cela!*' After many alternations he appeared at last so permanently prostrated that his family applied to his favourite pupil, M. Poisson, to try to get a word from him. Poisson paid a visit, and after a few words of salutation said, '*J'ai une bonne nouvelle à vous annoncer; on a reçu au Bureau des Longitudes une lettre d'Allemagne annonçant que M. Bessel a vérifié par l'observation, vos découvertes théoriques sur les satellites de Jupiter.*' Laplace opened his eyes and answered with deep gravity, '*L'homme ne poursuit que des chimères.*' He never spoke again. His death took place March 5th, 1827."

† "English Encyclopedia." The writer (De Morgan?) adds, the preceding comparison is drawn from Bossut (*Hist. des Math.*), who was the personal friend and the decided eulogist of both. He adds that the polished character of Clairaut procured him an *existence* and a consideration in the great world which talent alone would not have sufficed to gain; and more than insinuates that dissipation destroyed his constitution."

astronomy should not be deficient in politeness and good manners, something more may be expected from him, as springing from inward principle. Would it be too much to affirm that the teaching of the stars inculcates virtually a generosity of mind, tenderness towards the feelings of others, unaffected good-nature, and a disregard of petty personal considerations? That, on the other hand, it is opposed to assumption, jealousy, an overbearing spirit, and any kind of self-magnifying? It is painful to find a great mathematician remarkable for egotism, and rarely mentioning his predecessors; but happily we have no difficulty in pointing to other instances in which astronomical science was combined with many admirable qualities. From amongst these let us here refer only to Olynthus Gregory and Francis Baily. The former displayed early proficiency in mathematics, and was one of the founders of the Royal Astronomical Society. After noticing his pleasing manners, his biographer says:—"All he did and said was dictated by benevolence of feeling, and he was a man of unbounded charity. As a Christian, he was moral and devout; and as a scholar, he merited and obtained the consideration of the first mathematicians of the day: his great zeal in his vocation, his parental kindness, his earnest and impressive admonitions, his entertaining, improving, and philosophical conversation, and his ever-readiness to assist, will be gratefully remembered by many.*" Whilst the mathematical genius of some, like Maclaurin and Clairaut, was conspicuous in early youth, Baily, though he had a previous good knowledge of elementary mathematics, was fifty-one years of age when he devoted himself to astronomy. He also was one of the founders of the Royal Astronomical Society, and till his death, at seventy-two, he rendered to the science most important services. "He was distinguished by great industry, which was made more effective by his methodical habits, and also by a suavity of manner which greatly enlarged the circle of his friends. In the last twenty years of his life, he did much more and better work than younger men of much greater original power have done in twice the time—and besides this, his admirable personal qualities, which made it so easy to co-operate with him, led to his being able to make others do more than they could have done without him. His high moral worth added to his power; and all these together made him the most influential member of the astronomical world The writings of Francis Baily are models of patient thought, all but impeccable accuracy, careful research, and well chosen objects.†"

It is not pretended that the study of astronomy is necessarily

* "English Encyclopædia."

† "English Encyclopædia."

accompanied by such dispositions and manners as we have dwelt upon. Even vital Christianity not unseldom is but faintly illustrated by the actual graces which properly belong to it, and *ought* to be inseparable from it. An arrogant or unamiable, a cross-grained, ill-tempered, or unmannerly astronomer, are conceivable phenomena; but the tendency of the pursuit is, nevertheless, counter to these faults, and it should exercise a silent influence on his conversation, his correspondence, and (should he have any) his controversies. Hasty and heated language is sometimes pardonable: few indeed have not at some time or other thus erred; and the effect of it may pass away; but there is less excuse for objectionable matter of any kind in writing; less still in print. Often, no doubt, would the author have been glad at a later period to expunge such matter from his writings; but *littera scripta manet*, the blot is indelible, and may last as long as his fame. And here we may observe the good taste which distinguishes the writings of some eminent in science: there is a calmness, a dignity, very little of what is caustic, sarcastic, ironical; none of that smart writing which some mistake for wit, and which is out of place in discussing scientific subjects; a careful avoiding of misrepresentation, and a light touch when anything of a personal nature is in question: candour in admitting an error, and readiness to make the best rather than the worst of an opponent. Even when some sharpness and severity are called for, these are tempered and moderate. It is pleasant when we find personal matters made as little prominent as possible, and subordinated to the catholic interests of science, and the dictates of good feeling and good taste.

The last caution to be given is against a love of notoriety and spurious fame. It is quite possible to over value *deserved* fame, without subscribing to the extreme sentiment of D'Alembert's foster-mother, who used to say to him when he told her of any new work or discovery of his own, "You will never be anything but a philosopher—and what is that but a fool who plagues himself all his life, that he may be talked about after he is dead." No right-minded student, however, will seek to push himself unduly forward, or attach any value to that kind of ephemeral celebrity which is sometimes not difficult to obtain. The biographer of Bradley, in the work already several times referred to, says:—"Let any man set up for the founder of a sect, and begin by asserting that he has found out the cause of attraction, or the structure of the moon; let him exalt himself in the daily papers, and he must be unfortunate indeed, if in three years he is not more widely known in this country than its own Bradley, one of the first astronomers of any."

Cicero, in the well-known "Somnium Scipionis," relates that P. Cornelius Scipio dreamed he was with his grandfather, Africanus, on the circle of the Milky Way, and thence he beheld the stars, planets, sun and moon, and the Empire of Rome, which seemed a mere point on the, in itself, small earth, and his ears were filled with the music of the spheres. Africanus, seeing his eyes, amidst the surrounding glories, still fixed on the humble home of man, said to him—"If this abode appears to thee (as indeed it is) small, ever contemplate these celestial things, and despise those human ones:" and he proceeded to show him in how small a region, and for how short a time, he could expect his glory to be spread abroad in the world; and to exhort him to rest his hope, not on earthly fame and human rewards, but on the imperishable glories which should be the recompense in a future state of following the allurements of virtue, regardless of the discourse of others. And with far more light on the topics of this agreeable fiction than its writer possessed, we may yet profit by the lesson it inculcates, and learn, even from the splendours of the physical heavens, how little account is to be made of much that in this present scene so many are eagerly pursuing. Borne on waves of light from unnumbered stars and clusters, there comes to the thoughtful observer the admonition of Africanus,—"*HÆC CÆLESTIA SEMPER SPECTATO; ILLA HUMANA CONTEMNITO:*" a lesson taught in the stillness of the night, which should be remembered in the sun-bright hours, when there are so many calls for its application.

THE PLANET VULCAN.

The *Chicago Times* publishes an account of Professor Watson's observation of the Intra-Mercurial planet, given by him to a reporter of that journal, who had interviewed him. Having stated that he went to Wyoming "for the exclusive purpose of looking for the supposed planet between Mercury and the sun," he went on to say, "I found a star of 4½ magnitude, which had a decided disc, and was in a position in which there is no known star. I looked particularly to see whether there was any elongation such as would be expected if it were a comet in that position with respect to the sun, but it had none. The brief period of the observations did not permit me to change the power of the telescope. Having found the object, it was important to get its position. The method which I employed obviates entirely the danger of error from wrong circle readings. In place of the usual hour and declination circles, I substituted disks covered with cardboard, and by means of a pointer the place of an object was marked by a sharp pencil. In this way I recorded the positions of the planet, a neighbouring star, and the sun. And these paper circles can now be placed upon a dividing engine and read with entire accuracy. Professor Newcomb, of Washington, and Mr. Lockyer, of England, verified the relative positions of the sun and the planet, as shown on these circles before the instrument was disturbed. Until I return to Ann Arbor I cannot make any calculations to determine

whether this object is identical with the reported small spots seen upon the sun, which may have been due to the transit of an Intra-Mercurial planet across its disk. I have made an approximate measurement of the paper circles, and the place of the planet is found to be, right ascension, 8h. and 26m., declination, 18° N."

In reply to the remark, that "other astronomers were out there searching for the planet," but failed to see it, Professor Watson said, "They had either too small optical power to see objects of that degree of brilliancy, or else so high a power that in shifting from one field to another they passed over such an object, having no reference point in changing the field. I used a power of 45, with a telescope of four inches aperture, and before searching I had committed to memory the relative position of all the stars down to the seventh magnitude in the vicinity of the sun's place."

He thinks that should the elements of the new planet's orbit be determined, "it will be possible, with suitable contrivances, to exclude the direct light of the sun, to observe it near its elongation, using, of course, a telescope of great aperture and a high magnifying power. Mr. Ranyard, of England, has informed me that he is pretty sure that he has seen the place of the planet on one of the photographic plates, taken during the totality. These will be carefully examined for that purpose. The planet shone with a reddish light, which would give it less actinic power than in the case of a star."

He believed that he would see Vulcan. "I was satisfied that there was a planet within the orbit of Mercury, just as I am satisfied that there is outside the orbit of Neptune. The perturbations of those planets and some other phenomena cannot be explained on any other hypothesis. So when I went there I fixed on my plan and stuck to it. I determined to sweep south of the sun, and to keep within a small space. We had but $3\frac{1}{2}$ minutes, and the time was too short to try to get over too great a space. I meant to search that much thoroughly, and so reduce the amount for future astronomers should I not succeed. It was on the fifth sweep that I saw the object. It seemed about as large as a two-shilling piece, did not have so many rays as a star, and was without the elongation of a comet. As soon as I saw it my attention was attracted. I next felt certain I had found what I was looking for."

He was glad to learn that Professor Swift, of Rochester, thinks that he also had found the same object, for "Swift's discovery strengthens the facts of the planet's existence and its discovery."

The following is Mr. Swift's account of his observation of this planet at Denver City, as given to the *Rochester Democrat*:—

"About one minute after totality two stars caught my eye, about three degrees, by estimation, south-west of the sun. I saw them twice and attempted a third observation, but a small cloud obscured the locality. The stars were both of the fifth magnitude, and but one is on the chart of the heavens. This star I recognised as Theta in Cancer. The two stars were about eight minutes apart. There is no such configuration of stars in the constellation of Cancer. I have no doubt that the unknown star is an Intra-Mercurial planet, and am also inclined to believe that there may be more than one such planet. . . . I possessed a comet eyepiece of very flat and large field, and distinct to the very edge. . . . The corona was unusually extensive. The greatest prolongation was in the direction of the moon's path across the sun, and, as drawn by some of the parties, extended on each side of the sun to a distance of more than three million miles. The pencils of light were radial mostly, though some of them were curved."—*Times*.

REVIEWS.

The Sun.—*Diminished Light and Heat of the Sun the Cause of the Glacial Epoch, the Submergence and other Phenomena.* By J. Scott Moore. Dublin, & Rivingtons, London. 1878. pp. 10, with a Table.

The vicissitudes of climate of former geological periods as due to geographical change and astronomical causes, which readers of Lyell's "Principles" will remember are pretty fully treated by him, are passed by in this brochure, in which they are referred solely to frequent and sometimes periodical outbursts of heat and light in the sun (like what has taken place in certain stars) followed by intense cold. This theory may, possibly, not be without some truth taken in connection with other considerations, but it is certainly not presented to advantage by Mr. Moore in these few perfunctory pages, written in a slipshod style and sometimes with a peculiar orthography.

Hints on Reflecting and Refracting Telescopes. By W. H. Thornthwaite, F.R.A.S. Third edition.

Mr. Thornthwaite has issued a third edition of his useful little guide to reflecting telescopes, containing "Hints" and "Appendix" (which were formerly published separately), in one, with much additional matter.

The book opens with a short but lucid account of the various forms of reflecting and refracting telescopes, and the methods of adjusting them, and a minute description of the methods to be employed in centring the large and small specula of a Newtonian. Some space is devoted to a description of the different stands, especially of the "Berthon" equatorial; a cheap and useful, but singularly hideous, method of mounting. Mr. Thornthwaite gives all the details of the various processes of silvering a glass speculum, so clearly that the tyro should find no difficulty in accomplishing this for himself. The Berthon observatory and a cheap astronomical clock are next described, with some general hints on observation; we must demur, however, to the statement that Darby's "Astronomical Observer" is a very useful work: we should have considered it just the contrary. There must, by the way, be some mistake in the statement, on page 80, that ϕ Draconis and γ^2 Andromedæ were well divided at the beginning of 1877 with a $5\frac{1}{2}$ -inch stop on an 8 $\frac{1}{4}$ -inch mirror; inasmuch as the distance between the two components of the latter star was then about four-tenths of a second of arc.

The book seems remarkably free from errors of the press, the type is excellent, and there is almost a superabundance of illustrations.

British Rainfall, 1877. On the distribution of Rain over the British Isles, during the year 1877; observed at about 2,000 stations in Great Britain and Ireland, with map and Illustrations. Compiled by J. G. Symons, F.R.S., Secretary to the Meteorological Society, Membre de la Société Météorologique de France, Member of the Scottish Meteorological Society, &c. London: E. Stanford, Charing Cross, S.W.; Simpkin, Marshall & Co., Stationers' Hall Court. 1878. pp. 164. 5s.

Again the indefatigable Secretary of the Meteorological Society presents us with his elaborate and interesting report of the rainfall, based on the co-operation of many persons of both sexes, who contribute their money and observations for this very useful purpose. A little more of the former, we find, would be desirable; and in some places more observers are wanted. The extremes of rainfall in 1877 were these:—

Largest rainfall at Seathwaite, in Cumberland 180.40 in.
 Least rainfall at Inchkeith, Edinburgh 17.55 in.
 The map represents the site of rain gauges in Cumberland and Westmorland. There is a description with an illustration of the author's Illuminated Storm Rain Gauge, made by M. Pastorelli, for facilitating accurate observations of the fall of rain during thunderstorms; of great importance in drains and other questions.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER,

THE SUSPECTED NEW CRATER NEAR HYGINUS.

Sir,—A friend writes, "In the *Astronomical Register* a correspondent points out mistakes in your moon article (*Contemporary Review*); as you are not likely otherwise to notice his letter, I think it well to call your attention to it." I find my friend is right. I took the date of Klein's first observation from *Nature*, and turned to the *Nautical Almanac* to see if Klein observed near the first or third quarter, and through one of those pieces of carelessness we are all liable to, took out the epoch of "full" as "third quarter;" otherwise I should have detected the mistake in the date given in *Nature*. If the point had had the slightest bearing on the question I was dealing with, I should have been more careful. There was not in reality the least occasion to verify the date; and the point being utterly unimportant I did not pay much attention to it. I compliment your author on his acumen in noting this clerical error. He commits a mistake himself, with rather too obvious a purpose, in saying that I "calculated" my erroneous results. I dare say he is right in the meaning he ascribes to Mädler's words "very deep" as applied to Linné; but as Mädler gave no measure this must be a matter of opinion. He will hardly say that in exaggerating the original estimate of the depth of Linné I was opposing his favourite theory that volcanic change has taken place. I was in fact putting the evidence for that theory as strongly as I could,—though I consider the theory untenable.

Having thus admitted the validity of your correspondent's corrections in these matters, I need take no notice of other remarks of his. To speak mildly, they are scarcely pertinent. He seems to consider the question of the new crater a personal one, and fails to see that I incline to the theory of change rather than to that opposite view which so troubles him. I was not, however, thinking of him or his theory when I wrote my article; but simply proposed to myself to give the general reader an account of the supposed discovery. How I have done "an immense amount of mischief" in calling the attention of thousands to an interesting subject of lunar study, which I imagine that not many tens would have heard of through the *Selenographical Journal*, whatever that organ may be (I hear of it now for the first time), I leave your correspondent to expound, which he may do in the perfect assurance that I shall not be at the pains to comment on his exposition. Albeit I thank him for calling my attention to errors, however trifling, in a paper of mine on scientific matters.

RICHARD A. PROCTOR.

ROGER BACON AND THE TELESCOPE.

Sir,—To the interesting notes of the life of Friar Bacon, given in September's *Register*, a few words might be added on his knowledge of the result of the combination of lenses. The reputed discovery of magnifying lenses amid the ruins of Nineveh, the remains of an apparent lens in a lapidary's shop amid the débris of Pompeii, the ideas of Vitello, Baptista Porta, and others show how the principle of the telescope was conceived before the days of Galileo; and it will not lessen the debt we owe to the illustrious Florentine, if we remark that our own old English worthy had, probably, the clearest ideas on the subject of anyone before his day. The following from Dick's "Practical Astronomer," part ii., Ch. I., may be quoted in proof of this. Dr. Dick "says:—"Among the moderns the illustrious Friar Bacon appears to have acquired some rude ideas respecting the construction of telescopes. 'Lenses and specula,' says he, 'may be so figured that one object may be multiplied into many, that those which are situated a great distance may be made to appear very near, that those which are small may be made to appear very large, and those which are obscure very plain; and we can make stars to appear wherever we will.' From these expressions it is highly probable that this philosopher was acquainted with the general principle both of telescopes and microscopes, and that he may have constructed telescopes of small magnifying power, for his own observation and amusement, although they never came into general use."

Upton Helion's Rectory, Orediton:

Faithfully yours,

Sept. 18.

S. J. JOHNSON, F.R.A.S.

JUPITER ON JULY 31, 1878.

Sir,—In answer to H. J. Slack's letter on page 234, I may state that on July 31, I took a sketch of Jupiter at 10.18 p.m., with a Cooke's refractor of 4½-inches, and again observed it at 11.0. Definition was very good, but I did not notice any changes in the appearance of the planet. But as this information is only negative, it is no proof that changes did not take place. Of course there is a motion from f to p , resulting from the rotation of the planet.

Sunderland:

Sept. 12, 1878.

Yours truly,

T. W. BACKHOUSE.

ELECTRICITY AND THE SOLAR SYSTEM.

Some time ago I sent to you my memoir on "the Electric Constitution of our Solar System." That memoir has received a remarkable confirmation from the recent solar eclipse. One observer, General A. J. Myer, the chief of the weather map department of our Government, says, "the corona was traced several diameters from the sun, and seen five minutes after totality." Another observer, Prof. Langley, of the Alleghany observatory, says he could see "the corona to the distance of twelve diameters of the sun on one side, and three diameters on the other." These are both reliable men, and all the others say it was the largest and brightest corona ever seen.

These observations confirm my statement of facts in my memoir, that the solar corona is eminently variable, and therefore like our aurora borealis, which is known to be electric. The far-off projecting streamers form another striking resemblance.

The outward projection as far as twelve diameters from the sun

amounts to ten millions of miles. This vast electric mass must have a great electric repulsion through vacant space, and it lends probability to my position that it drives away from the sun the tails of comets and our zodiacal light and aurora borealis. If the corona could be distinctly seen reaching off 10,000,000 miles from the sun at our distant stand-point, it could probably have been seen double that distance from a nearer position—therefore its repulsion might affect the most distant tail of a comet. Electricity alone can repel electricity. And I think that in my memoir, along with this recent coronal manifestation, there are facts enough to prove that the direction of the comets' tails is but the interaction between the sun and the comets, the same as the action between a charged prime conductor and a charged pith ball of an electric machine.

Normal School, Shippenburg, Pa:

JACOB ENNIS.

Aug. 7, 1878.

JUPITER'S BRIGHT SPOTS.—MOON ECLIPSE.

Sir,—I enclose an extract translated from a letter with which I have been favoured by M. L. Niesten, Assistant Astronomer at the Royal Observatory Brussels. The sketches to which this note refers correspond very well, especially that of 24th August, with the spots I saw. I have noticed on several occasions that the roundest and brightest spots were near the centre, but I have not had an opportunity of observing the elongations as they approach the margin of the disk, as plainly as M. Niesten has done.

To do this requires a power such as he employed ($\times 180$); I have seldom, and then for only a little while, found the weather good enough for so much magnification. Generally, if not cloudy, there has been a strong cold wind, making vision unsteady.

Passing from Jupiter to the moon, I should have sent a few remarks on the eclipse of 12th August, but I suppose other correspondents would have supplied full notes. At 9.22 p.m. the penumbra, as seen here, with $6\frac{1}{2}$ silvered mirror, was sooty and so opaque as to make the brightest objects it passed over quite invisible; Tycho and Copernicus included. Cirrusstrative clouds interfered with observations, but the shadow alone never made the border of the moon's disk too obscure to be seen faintly. The middle of the eclipse was at 12.8 p.m., and shortly before (11.55) Copernicus became faintly visible as a pale white spot. By 12.15, most of the umbra had turned coppery, but not bright in tint, or transparent. All through the eclipse the umbra was darkest at the approaching margin for a width about equal to that of Copernicus. When the rest of the shadow turned coppery this band looked blue. The observations were made with Huyghenian eye-piece, $\times 80$.

Ashdown Cottage, Forest Row,

I am, &c.,

Sussex: Sept. 21, 1878.

HENRY J. SLACK.

(Translation.)

From M. L. Niesten, Assistant Astronomer, Royal Observatory, Brussels, 16th September, 1878.

Sir,—In reply to the request you addressed to observers relating to the aspect of Jupiter on the 31st July, between 10 and 11 o'clock, p.m., allow me to send you two sketches of drawings I made of the planet under a meridian adjacent to that of your observation.

On the 31st the sky was cloudy at Brussels, and I made no observation, but perhaps in the enclosed drawings you may recognise the spots of 31st July.

In the numerous series of drawings which I have made of the planet during this opposition, the aspect of the equatorial zone has struck me by the succession of round, white, luminous spots which it has presented. These spots elongate as they get towards the borders of the disk, and are rounder and more brilliant as they approach the centre. The drawings sent correspond with 15 and 342 degrees of Mr. Marth's ephemeris, published in the *Astronomical Register*. On 31st July at 10h. p.m. the view was about 341° , and at 10h. 45m. 13° .

TWO RECENT COMETS.

Sir,—The following ephemeris of Tempel's comet of 1873, for the month of October, 1878, has been calculated by M. Schulof:

1878.		R.A.			Decl.	Brightness.
		h.	m.	s.		
Oct. 1	...	18	33	23	... -30°	39
2	...	18	37	22	... 30°	47
3	...	18	41	22	... 30°	54
4	...	18	45	23	... 31°	1
5	...	18	49	24	... 31°	7
6	...	18	53	25	... 31°	13
7	...	18	57	27	... 31°	18
8	...	19	1	29	... 31°	23
9	...	19	5	31	... 31°	27
10	...	19	9	33	... 31°	30
11	...	19	13	36	... 31°	33
12	...	19	17	38	... 31°	35
13	...	19	21	41	... 31°	37
14	...	19	25	41	... 31°	37
15	...	19	29	42	... 31°	38
16	...	19	33	43	... 31°	37
17	...	19	37	44	... 31°	37
18	...	19	41	43	... 31°	35
19	...	19	45	42	... 31°	34
20	...	19	49	41	... 31°	31
21	...	19	53	38	... 31°	28
22	...	19	57	35	... 31°	25
23	...	20	1	31	... 31°	21
24	...	20	5	26	... 31°	17
25	...	20	9	19	... 21°	12
26	...	20	13	12	... 31°	6
27	...	20	17	4	... 31°	0
28	...	20	20	54	... 31°	54
29	...	20	24	44	... 30°	47
30	...	20	28	31	... 30°	40
31	...	20	32	18	... 30°	32

The comet is slowly receding, both from the earth and the sun.

The following are revised elements, also by Schulof.

P.P. = 1878, August 27^o. Berlin M.T.

π	=	306°	7
Ω	=	121	0
i	=	12	46
ϕ	=	33	37
μ	=	682	
Log. a	=	0.477358	

The comet found in America by Swift on July 7, has not been generally seen in Europe, because of an error in the telegram conveying the intelligence of its discovery. It has now passed beyond reach.

The following provisional elements are by Holetschek, of Vienna:

P.P. = July 21²⁶26. Berlin M.T.

π = 280° 19'

Ω = 102° 18'

i = 78° 0'

Log q = 0.14328

Its motion, both southwards and away from the earth, was very rapid.
Northfield Observatory, Your obedient servant,
Eastbourne: Sept. 3, 1878. G. F. CHAMBERS.

MINOR PLANET CERES.

The minor planet, the first of the series, comes into opposition on the 29th of the month. The following is its position over the meridian of Greenwich:

	R.A.			Decl.		
	h.	m.	s.	°	'	"
Oct. 21	2	36	54.84	2	57	30.6
29	2	29	47.09	2	38	19.7

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN OCTOBER, 1878.

By W. R. BIRT, F.R.A.S., P.S.S.

HYGINUS N. The new Crater.—In the last number of the *Register*, page 230, Mr. Neison solves the problem of Mr. Hutchings' elliptical depression, by referring it to the valley between Hyginus and Hyginus ν which is laid down very plainly in Map II. of his moon, but not so distinctly in the Outline Diagram of the Selenographical Society, May 2, 1878. Miss Ashley in describing her observations of July 14 to 20, says, "From the 14th to the 19th I saw an oval object which at that time I took to be Mr. Hutchings' oval, it seemed so nearly in its position and of its appearance; the interior was filled with shadow, and there were white spots on its border. Neither of these spots answer to any spots in Mr. Hutchings' drawing." Miss Ashley gives four white spots, three on the south-east, provisionally numbered from S.W. to N.E., 14, 16, and 18, and one on the north-west numbered 20 supposing them all to be craters. Miss Ashley says further—"as the region approached the terminator on the 18th, I saw that the oval was really formed of two parallel and very large ridges slightly turned at each end, so having the appearance of a perfect oval. One of these ridges came to a stop at a crater, but it was continued after that again." It appears from the drawings that this crater numbered 20 is on the north-west border.

In August Miss Ashley obtained some further observations of this region. She sends two sketches dated August 16 and 17, with alignments. In her description she says "the crater b (see Neison's map) is in a line with the Spiral mountain and the ring-shaped object on the large ridge." This ring-shaped object appears from her drawing of

July 14 to be number 20 of that sketch, but from her alignment on August 16 I am disposed to identify it with Hyginus ν . On August 17, her sketch shows this object in a line with Hyginus and Manilius, it being the point of intersection of this line with that passing through the Spiral mountains and the crater δ . The line from Hyginus ν through the Spiral mountains to the crater δ , crosses the Hyginus cleft north of two widenings of the cleft shown by Miss Ashley. These widenings are not shown by Neison, but the alignments are in agreement with the map. Schmidt in his large map has four complete craters (widenings) on the N.E. branch of the Hyginus cleft. His delineation of the Spiral mountain appears to be very imperfect, but the alignment δ : Spiral: ν is correct. If, however, Mr. Hutchings' elliptical depression be the valley which Mr. Neison speaks of, ν is situated on a ridge more to the north-west of the boundary of the valley. The delineation of the region in the neighbourhood of ν in Schmidt's is very complex, so much so that we repeat the remark at the close of our last list, page 235, viz. that "careful drawings of the region will be very important." Schmidt has a long valley south-east of the ridge on which ν is situated.

Stratford, E.: September 17, 1878.

THE PLANETS FOR OCTOBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	11 38 14	N. 4 21	6".0	22 54.1
	9th	12 27 28	S. 0 58½	5".1	23 11.8
	17th	13 17 54	S. 6 54	4".8	23 30.6
	25th	14 1 24	S. 11 49½	4".7	23 48.7
Venus ...	1st	11 36 22	N. 4 8	10".2	22 52.3
	9th	12 13 0	N. 0 12	10".1	22 57.3
	17th	12 49 41	S. 3 46	10".0	23 2.5
	25th	13 26 46	S. 7 40	10".0	23 8.0
Jupiter ...	1st	19 58 36	S. 21 22	39".5	7 17.1
	9th	19 59 56	S. 21 18	38".6	6 47.0
	17th	20 2 6	S. 21 11½	37".6	6 17.6
	25th	20 5 3	S. 21 3	36".7	5 49.1
Saturn ...	1st	23 59 32	S. 2 50	17".6	11 17.3
	9th	23 57 22	S. 3 4	17".5	10 43.7
	17th	23 55 21	S. 3 16½	17".4	10 10.3
	25th	23 53 33	S. 3 27	17".3	9 37.0
Neptune ...	4th	2 29 18	N. 12 48	...	13 34.9
	20th	2 27 41	N. 12 40	...	12 30.4

Mercury rises about an hour and a half before the sun on the 1st, the interval decreasing till the 24th when he rises about the same time as the sun.

Venus rises an hour and three-quarters before the sun at the beginning of the month, the interval decreasing.

Jupiter may be observed till forty minutes before midnight on the 1st, the interval increasing. On the last day he sets at 9h. 35m.

Saturn sets an hour and a half before sunrise on the 1st, the interval increasing.

ASTRONOMICAL OCCURRENCES FOR OCTOBER, 1878.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Tues	1	7 41	Occultation of 43 Ophiuchi (6) Sidereal Time at Mean 12h. 40m. 20' 49s.	2nd Ec. R.	6 7 41	— Saturn.
Wed	2	19 0	☾ Moon's First Quarter	1st Tr. I. 1st Sh. I.	9 59 11 15	11 13' 1
Thur	3		Sun's Meridian Passage 10m. 57' 41s. before Mean Noon	1st Oc. D. 1st Ec. R.	7 19 10 53 30	11 8' 9
Fri	4	10 1 4	Near approach of 4 Capricorni (6) Conjunction of Moon and Jupiter 0° 34' N.	1st Sh. I. 1st Tr. E. 1st Sh. E. 4th Oc. D.	5 44 6 47 8 4 11 9	11 4' 7
Sat	5	8 54 10 7	Occultation of 8 Capricorni (4) Reappearance of ditto	3rd Tr. I.	10 6	11 0' 5
Sun	6			2nd Tr. I.	9 5	10 56' 3
Mon	7					10 52' 1
Tues	8			2nd Ec. R.	8 44 58	10 47' 9
Wed	9	11	Conjunction of Moon and Saturn 6° 59' S.	3rd Ec. D. 3rd Ec. R.	5 21 53 8 52 30	10 43' 7
Thur	10	20 54	☾ Full Moon	1st Oc. D.	9 12	10 39' 5
Fri	11	13 8 14 23	Occultation of 101 Piscium (6) Reappearance of ditto	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	6 30 7 39 8 40 10 0	10 35' 3
Sat	12	18 22 19 6 8	Occultation of 26 Arietis (6) Reappearance of ditto Conjunction of Mars and Mercury 1° 0' N. Saturn's Ring: Major axis=43" 64 Minor axis=1" 89	1st Ec. R.	7 17 42	10 31' 1
Sun	13	5 53 6 46 6 19	Occultation of 8 Arietis (4½) Reappearance of ditto Occultation reappearance of 27 Tauri (4)	4th Sh. I.	9 16	10 27' 0
Mon	14	6 21	Occultation reappearance of 28 Tauri (5½)			10 22' 3
Tues	15		Illuminated portion of disc of Venus=0.976 Illuminated portion of disc of Mars=0.998	2nd Oc. D.	5 47	10 18' 6

DATE.		Principal Occurrences.	Jupiter's Satellites.	Meridian Passage.	
		h. m.		h. m. s.	h. m.
Wed	16	Sidereal Time at Mean Noon 13h. 39m. 28 ^s .08.	3rd Oc. R. 3rd Ec. D.	7 34 9 23 27	Saturn. 10 14 ⁴
Thur	17	9 17 Occultation of α Gemini- norum (34) 10 9 Reappearance of ditto 19 9 ζ Moon's Last Quarter Sun's Meridian Passage 14m. 47 ^s .19s. before Mean Noon	2nd Sh. E. 1st Tr. I. 1st Sh. I.	6 33 8 15 9 35	10 10 ³ 10 6 ¹
Sat	19	13 47 Occultation of δ Cancer (4) 14 14 Reappearance of ditto 18 3 Occultation of 18 Leonis (6)	1st Oc. D. 1st Oc. R.	5 35 9 13 5	10 1 ⁹
Sun	20	19 7 Reappearance of ditto 21 Conjunction of Mars and Venus 0° 45' N.	1st Tr. E. 1st Sh. E.	5 4 6 24	9 57 ⁸
Mon	21	16 Jupiter at quadrature with the Sun	4th Oc. R.	9 28	9 53 ⁶
Tues	22		2nd Oc. D.	8 24	9 49 ⁴
Wed	23		3rd Oc. D.	7 58	9 45 ³
Thur	24	8 Superior conjunction of Mercury and the Sun 18 Conjunction of Moon and Mars 5° 54' N. 22 Conjunction of Moon and Venus 6° 40' N.	2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	6 14 6 28 9 10	9 41 ²
Fri	25	10 58 $\bullet$ New Moon 16 Conjunction of Moon and Mercury 5° 46' N.			9 37 ⁰
Sat	26		1st Oc. D.	7 31	9 32 ⁹
Sun	27		1st Sh. I. 1st Tr. E. 3rd Sh. E. 1st Sh. E.	5 59 7 0 7 1 8 20	9 28 ⁸
Mon	28		1st Ec. R.	5 37 18	9 24 ⁶
Tues	29				9 20 ⁵
Wed	30	4 2 Occultation of σ Sagit- tarii (24) 5 7 Reappearance of ditto 10 Opposition of Neptune to the Sun	4th Sh. E. 2nd Tr. I. 2nd Sh. I. 2nd Tr. E.	8 17 6 10 8 50 9 6	9 16 ⁴ 9 12 ³
Fri	1	NO V.			9 8 ²

JUPITER.

1878. oh. Gr.	Angle of position of $\mathcal{U}$'s axis.	Latitude of Earth Sun above $\mathcal{U}$'s equator.	Equat. diameter.	Defect of illumination.
Oct. 2	$346^{\circ}96$	$-0^{\circ}65$	$41^{\circ}77$	$0^{\circ}37$
12	$346^{\circ}78$	$0^{\circ}64$	$40^{\circ}48$	$0^{\circ}38$
22	$346^{\circ}48$	$0^{\circ}63$	$39^{\circ}23$	$0^{\circ}38$
Nov. 1	$346^{\circ}08$	$-0^{\circ}60$	$38^{\circ}05$	$0^{\circ}37$

The assumed First Meridian passes the middle of the illuminated disk at the following times :

1878. G. M. T.

Oct. 1	h. m.	Oct. 11	h. m.	Oct. 22	h. m.
1	4 2	9	12 5	3	17 0
	10 59 7	19	8 1	13	12 6
	20 55 2	12	5 3 6	23	8 1
2	6 50 7	14	59 2	23	9 3 7
	16 46 3	13	0 54 7	18	59 3
3	2 41 8	10	50 3	24	4 54 8
	12 37 3	20	45 8	14	50 4
	22 32 9	14	6 41 4	25	0 46 0
4	8 28 4	16	36 9	10	41 5
	18 23 9	15	2 32 5	20	37 1
5	4 19 4	12	28 0	26	6 32 7
	14 15 0	22	23 6	16	28 2
6	0 10 5	16	8 19 1	27	2 23 8
	10 6 1	18	14 7	12	19 4
	20 1 6	17	4 10 3	22	15 0
7	5 57 1	14	5 8	28	8 10 6
	15 52 7	18	0 1 4	18	6 1
8	1 48 2	9	56 9	29	4 1 7
	11 43 7	19	52 5	13	57 3
	21 39 3	19	5 48 1	23	52 9
9	7 34 8	15	43 6	30	9 48 5
	17 30 4	20	1 39 2	19	44 0
10	3 25 9	11	34 7	31	5 39 6
	13 21 4	21	30 3	15	35 2
	23 17 0	21	7 25 9	Nov. 1	1 30 8
		17	21 4	11	26 4

A. MARTH.

THE SATELLITES OF SATURN.

Major and minor semi-axes of the apparent orbits of the satellites, expressed in semi-diameters of Saturn's equator.

	Mimas.	Encel.	Tethys.	Dione.	Rhea.	Titan.	Iapetus
Major	$3^{\circ}14$	$4^{\circ}03$	$4^{\circ}99$	$6^{\circ}40$	$8^{\circ}93$	$20^{\circ}71$	$60^{\circ}34$
Minor { Oct. 2.	$0^{\circ}15$	$0^{\circ}20$	$0^{\circ}24$	$0^{\circ}31$	$0^{\circ}44$	$0^{\circ}94$	$8^{\circ}72$
Nov. 1.	$0^{\circ}11$	$0^{\circ}14$	$0^{\circ}17$	$0^{\circ}22$	$0^{\circ}31$	$0^{\circ}64$	$8^{\circ}23$

The centre of the apparent ellipse described by Titan may be assumed to be $0^{\circ}60$ semi-diam. east of the centre of the ball. The posit.-angles of its axes are $0^{\circ}56$, and those of the orbit of Iapetus on Oct. 2, $12^{\circ}35$, and on Nov. 1, $12^{\circ}17$ less than the pos. angles of those of the inner satellites.

Approximate Greenwich times of some of the conjunctions of the satellites with the ends of the ring, and also of some of their greatest elongations.—V. p. 189:

1878. Gr. M. Time.

		h.	
Sept. 30	8		Titan sup. o' with centre.
	11		" sf. limb.
	12.2		Tethys. np.
	13.2		Encel. sf.
Oct. 1	10.6		Encel. w.
	10.8		Tethys. sf.
	12.2		Dione. sf.
2	8.0		Encel. nf.
	9.4		Tethys. np.
	14.6		Encel. np.
	15.1		Dione. nf.
3	8.1		Tethys. sf.
	11.5		Dione. w.
	11.9		Encel. e.
	15.9		Tethys e.
4	6.7		Tethys. np.
	7.9		Dione. sf.
	9.3		Encel. sp.
	9.6		Rhea. sf.
	14.5		Tethys. w.
5	8.0		Rhea. e.
	8.3		Encel. np.
	8.8		Dione. nf.
	13.2		Tethys e.
	13.3		Encel. w.
6	6.5		Rhea. nf.
	10.6		Encel. nf.
	11.8		Tethys. w.
	15.7		Rhea. np.
7	9.6		Encel. sf.
	10.4		Tethys. e.
	14.0		Dione. e.
	14.2		Rhea. w.
	14.6		Encel. e.
8	7		Titan. nf.
	7.0		Encel. w.
	9.1		Tethys w.
	10.4		Dione. np.
	11		Titan. nf. limb.
	11.9		Encel. sp.
	12.7		Rhea. sp.
9	7.7		Tethys. e.
	10.9		Encel. np.
	11.3		Dione. sp.
	15.5		Tethys. nf.
10	7.6		Dione. e.
	8.3		Encel. e.
	13.3		Encel. nf.
	14.2		Tethys. sp.
11	12.3		Encel. sf.

1878. Gr. M. Time.

		h.	
Oct. 11	12.8		Tethys. nf.
12	9.6		Encel. w.
	11.4		Tethys. sp.
	12.8		Dione. sf.
	14.6		Encel. sp.
13	7.0		Encel. nf.
	10.1		Tethys. nf.
	10.2		Rhea. sf.
	12.6		Encel. np.
	13.8		Dione. nf.
14	8.7		Rhea. e.
	8.7		Tethys. sp.
	10.1		Dione. w.
	11.0		Encel. e.
15	7.2		Rhea. nf.
	7.4		Tethys. nf.
	8.3		Dione. sp.
	14.5		Tethys. np.
16	6.0		Tethys. sp.
	7.3		Encel. np.
	7.4		Dione. nf.
	8.1		Titan. sf. limb.
	12.3		Encel. w.
	12.1		Titan. sf.
	13.1		Tethys. sf.
	14.9		Rhea. w.
17	9.7		Encel. nf.
	11.8		Tethys. np.
	13.3		Rhea. sp.
18	8.7		Encel. sf.
	10.4		Tethys. sf.
	12.6		Dione. e.
	13.6		Encel. e.
19	9.0		Dione. np.
	9.1		Tethys. np.
	11.0		Encel. sp.
20	7.7		Tethys. sf.
	9.9		Dione. sp.
	10.0		Encel. np.
21	6.4		Tethys. np.
	7.4		Encel. e.
	12.3		Encel. nf.
22	10.9		Rhea. sf.
	11.3		Encel. sf.
	12.8		Tethys. e.
23	8.7		Encel. w.
	9.4		Rhea. e.
	11.5		Tethys. w.
	11.5		Dione. sf.
24	7.9		Rhea. nf.
	8.1		Titan. nf. limb.

1878. Gr. M. Time.

	h.		
Oct. 24	10.1	Tethys.	e.
	12.4	Dione.	nf.
	12.6	Encel.	np.
	14.4	Titan	np. limb.
25	8.7	Tethys	w.
	8.8	Dione.	w.
	10.0	Encel.	e.
26	7.4	Encel.	sp.
	7.4	Tethys.	e.
	13.9	Encel.	sf.
	14.0	Rhea.	sp.
27	11.3	Encel.	w.
	13.8	Tethys.	sp.
	14.0	Dione.	np.
28	8.7	Encel.	nf.
	12.5	Tethys.	nf.
29	5.5	Rhea.	np.

1878. Gr. M. Time.

	h.		
Oct. 29	7.7	Encel.	sf.
	11.1	Tethys.	sp.
	11.3	Dione.	e.
	12.6	Encel.	e.
30	7.6	Dione.	np.
	9.8	Tethys.	nf.
	10.0	Encel.	sp.
31	8.4	Tethys.	sp.
	8.6	Dione.	sp.
	9.0	Encel.	np.
	11.7	Rhea.	sf.
Nov. 1	6.3	Titan.	sf. limb.
	7.1	Tethys.	nf.
	10.1	Rhea.	e.
	10.3	Titan.	sf.
	11.3	Encel.	nf.

Careful observations of the conjunctions of Titan and of its distance from the major axis of the ring on Sept. 30, Oct. 8, 16, 24, Nov. 1 are most desirable. Iapetus reaches its inferior conjunction on Oct. 11, and its greatest western elongation on Oct. 31.

A. M.

Books Received.—De l'origine et de l'établissement des mouvements astronomiques. Par M. C. Lagrange, Bruxelles: F. Hayez. 1878.—Description of Observatory at Higher Bebbington, Cheshire. By R. C. Johnson.—The Stars in their Courses. By T. S. Baxley. London: Trübner & Co. 1878.—Micrometrical Measurements of 517 Double Stars at Cincinnati Observatory. O. Stone, Director. Cincinnati: Peter Thornham. 1878.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec. 1877.	To Nov. 1878.	To March, 1879.
Green, S.	Hartree, Dr.	Court, J.
To June, 1878.	To Dec., 1878.	To Aug. 1879.
Heelis, J.	Metcalfe, Rev. W. R.	Kelly, W. F.
Ryves, E. W.	To Jan., 1879.	To Dec., 1879.
	Shaw, Rev. J.	Thompson, Prof. D.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for the Editor are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The Astronomical Register.

No. 191.

NOVEMBER.

1878.

REMARKABLE CHANGES ON THE MOON'S SURFACE.

BY DR. HERMANN J. KLEIN.

(Translated from *Sirius*, 1878, p. 79.)

In the central and best seen portion of the disk of the moon, between the crater Hyginus and the walled plain Boscovich, there is a dark district, flat towards the east, but in the west intersected by a series of parallel chains of hills, which at the full moon are remarkable for their unusually dark colour. This region is bounded eastwards by the great furrow of Hyginus, at the west bank of which there is a curiously curved mountain, or rather system of hills which, as Mädler very characteristically describes it, presents the appearance of a spiral arrangement. The first person to make a drawing of this neighbourhood was Schröter, who has, speaking generally, indicated correctly the position of the western grey chains of hills, as also the course of the large Hyginus furrow. At the position of the Spiral Mountain his drawing represents a sickle-shaped range of hills together with a crater, whereby one perceives that he saw the mountain imperfectly. Lohrmann gave a more accurate representation of the entire district on the first sheet of his map of the moon; Mädler, however, a few years afterwards gave a still better one, inasmuch as he published a special map of the neighbourhood of Hyginus on a scale twice and a half as large as that of his great lunar map. This special map represents the district in question very correctly, speaking in a general way, and contains a vast number of small craters and low ridges of hills which had

VOL. XVI.

escaped earlier observers. The great lunar map of Schmidt, in Athens, agrees admirably with Mädler's special drawing, and in the level district westward of Hyginus contains in its turn a few additional craters, which are, however, the minima of visibility, and to be made out only when near the terminator in very fine instruments. The district of the moon's surface here spoken of, which at every lunation is to be observed with equal distinctness at the time of the first or last quarter, has consequently been as thoroughly investigated and sketched as it is well possible to imagine, and it can admit of no doubt that should a crater *larger and more distinct than the other ones* make its appearance here, it may be inferred, with a certitude that cannot be surpassed, that we have to do with a recent formation.

Now this is the case that has occurred. For nearly eleven years I have occupied myself specially with observations of the moon, and in doing so have on numberless occasions viewed the neighbourhood round about Hyginus without noticing objects that were in variance with the recognized maps. On the 19th of May, 1877, I examined, after a shower of rain, the flat region to the west of Hyginus, employing powers of 135, 200, and 300, and at once caught sight here of a large black crater filled with shadow, without any wall, which, *from my intimate acquaintance with this district, I at once recognized as one that I had never seen as yet.* The moon was not as clear as it usually is, and of the small craters in the grey plain there was but one that was visible, to the south of the new one, and considerably smaller than that was. Unfavourable weather prevented a continuance of the observations, but on the 18th of July an opportunity of examining the new crater was again offered. The air was not very good, the moon, going down and boiling. I was nevertheless able to see distinctly the small craters in the plain to the west of Hyginus, and the shadows of their walls could indeed be made out. The new crater was seen as a large black spot surrounded by a vapourous border. Only in favourable moments was a sharply circumscribed nucleus to be recognized as distinguished from the undefined border. The whole, to a person who had been long familiar with the objects seen in the moon, presented an excessively mysterious appearance, from being much darker than the dark spot indicated by Mädler at a certain distance towards the west. Later on I imagined I made out, in addition, a small, hazy, circular spot which was not visible at the commencement of the observation. On the following day, the 19th of June, the small craters in the plain were seen distinctly and without difficulty, whereas the new, large crater had disappeared. At the place it had occupied there was *perhaps* seen an undefined

grey spot having nothing of the nature of a shadow about it. A power of 200 was had recourse to.

After a long spell of unfavourable weather, such as is not met with in the lapse of many years, it was not until half-past seven in the evening of the 13th of November that an opportunity was offered for the examination of the vicinity of Hyginus with a power of 300. The air was tolerable, and those who are versed in lunar conditions of distinctness will know how the matter stood when I mention that the crater-like enlargements of the Hyginus furrow were at once to be distinctly seen. At the place of the new crater there was seen a large spot surrounded by an ill-defined border, the central portion whereof was black like a shadow. With a power of 200 the spot was caught up at the first glance, and I estimated its diameter to be two-thirds of that of Hyginus, that is to say, about 4,400 yards. On the 14th of November, the air being remarkably clear, not a trace of the crater was to be perceived. The moon was seen on the 13th of December with exceptional clearness and calmness, yet with powers ranging from 135 to 420 all that could be made out of the new crater was a round dull blackish spot hardly distinguishable. On the 10th of January, 1878, the terminator passed over the new crater; unfortunately I had to abandon the observation when it had just come to touch the western edge thereof. The entire country hereabouts appeared covered with extraordinarily numerous very small scattered hills or rubbish.

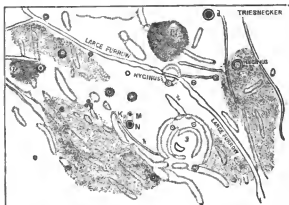
At 8 o'clock on the 15th of March, when the terminator was situated eastward of Herodotus and Mersenius, the central regions of the moon being consequently exposed to illumination under a somewhat considerable altitude, Hyginus and its furrow were seen with great distinctness. All the inequalities of the furrow were revealed by the greater or lesser capability of reflecting the light, and the course of the furrow through Hyginus itself shone like a fine line. With a power of 300 I saw in the grey plain to the west of Hyginus all the small craters I am acquainted with as luminous patches. There was, on the contrary, no trace of the new crater; its position even was not indicated by the dulllest spot.

The remarkable spiral hill spoken of above has received a broad furrow, or indeed a valley, towards the south. It is very easily made out, under an illumination of considerable altitude it presents almost precisely the appearance of the Hyginus furrow. At the terminator in the waxing moon it may be mistaken for the Hyginus furrow when the latter has not yet emerged from the unilluminated portion. This valley-like furrow is wanting in Schröter, Lohrmann, and also in Mädler's special map.

M. Schmidt, the director of the observatory at Athens, was so good as to favour me with a copy of the part in question of his large map of the moon. Here, too, the valley is entirely wanting, as is also the new crater. Subsequently to a communication from me M. Schmidt has seen and made a drawing of those objects.

*DISTRICT SURROUNDING THE NEW CRATER
NEAR HYGINUS.*

(From *Sirius*, No. 8, 1878.)



- N The new crater.
- M Tongue-shaped prolongation, and depression towards the south.
- S Mädler's spiral hill.
- t Large recently-formed valley.
- p, p1 Dark spots in the grey plain.
- H, K Small furrows.

For the present I abstain from offering any suggestions with regard to the important changes that have taken place on the moon to the west of Hyginus, as set forth above. Perhaps the series of changes there is not yet concluded. On the other hand, I would request such observers as are provided with instruments of sufficient power to direct their attention to the new formations in question.

Cologne: March 17, 1878.

DR. HERMANN J. KLEIN ON THE NEW CRATER
NEAR HYGINUS.

(From *Sirius*, 1878, p. 114.)

On the 19th of April of the present year the state of the weather was such as to admit of the new crater being observed. About 20 minutes after 7, while it was yet light, it was to be seen extremely distinctly, and quite as obviously as the crater of Hyginus itself. The terminator of the waxing moon passed through Triesnecker. The slight depression showed no wall, but was surrounded with a brown greyish border, which was prolonged in a tongue-shaped form towards the south, where a small black opening is likewise seen. At times the crater appeared perfectly black and distinct, and I conceive the grey border (the penumbra) to be simply the slope of the funnel-shaped precipice. Yet, supposing this to be the case, it seems strange that, when the altitude of the sun is more considerable, the funnel-shaped declivity is equally bright all round. For the rest, I had an impression that the crater, properly speaking, became veiled at intervals, for minutes at a time, or in other words appeared hazy. The new valley at the remarkable spiral hill near the Hyginus furrow was excessively distinct; its sides are undulating, and it strictly resembles the Hyginus furrow, although perhaps not so deep. After 8 o'clock, the images being signally good, after long and careful examination, I recognised in the plain to the west of Hyginus numerous craters which were minima of visibility, in addition to those I am familiar with; they are, however, too dull to be able to enter their position on the map with certainty.

The new crater is of a deep black, and well-defined. Several low ranges of hills extend from it in a westerly direction; between these and the crater itself there is to be made out an extremely narrow furrow with sharp borders. It is decidedly one of the most minute that I have ever seen on the moon, but it is apparently deep, for the shadow of one of the slopes is distinct. The furrow runs parallel to the hill. Between it and the new crater there is further to be seen, in favourable moments, a small portion of a second furrow. It is, however, very difficult to see. At 9 o'clock the new crater had become decidedly duller, and in its interior did not any longer appear as black as the shadow of Hyginus, but dark brown. On the other hand, the furrow situated towards the west gave a sharp, deep-black shadow, and I made out that it originates in the southern declivity of the spiral hill. The new crater is larger than Hyginus *b*, but smaller than Triesnecker *d*.

The cleft between the Ariadæus furrow and the Hyginus furrow was again visible on this evening. I also made out again that the Hyginus furrow has two branches to the north of Agrippa, and that it is by their union that the main furrow is formed. Upon the evening in question powers of 200, 300 and 420 were used.

On the following day, the 10th of April, between 8 and 9 o'clock in the evening, the new crater presented the appearance of a grey diffused spot, without central inmost shadow, the southern tongue-shaped prolongation remained still visible.

REVIEWS.

Popular Astronomy. By Simon Newcomb, LL.D., Professor, U. S. Naval Observatory. With 112 engravings, and five maps of the stars. London: Macmillan & Co. 1878. pp. 566.

The main object of this work is "to present the general reading public with a condensed view of the history, methods, and results of astronomical research, especially in those fields which are of most popular and philosophic interest at the present day, couched in such language as to be intelligible without mathematical study." (Preface.) It is divided into four parts: 1. *The system of the world historically developed*, comprising the ancient astronomy—the Copernican system—universal gravitation. 2. *Practical Astronomy*, comprising the telescope—application of the telescope to celestial measurements—measuring distances in the heavens—the motion of light—the spectroscope. 3. *The solar system*; comprising the general structure of the solar system—the sun—the inner group of planets—the outer group of planets—comets and meteors. 4. *The stellar universe*; comprising the stars as they are seen—the structure of the universe—the cosmogony. And there is an appendix containing a variety of tables and useful information, a glossary, and five maps of the stars to the fifth magnitude inclusive, and a general index. The frontispiece represents the great telescope of the U. S. Naval Observatory, Washington, constructed by Alvan Clark & Sons, 1873, with which some of the views of planets and nebulae illustrating the work were taken.

This is a very excellent book. It is likely to please various sorts of readers. One to whom the subject is new could scarcely have a better introduction. It will enlighten those whose ideas on some parts may be hazy. And they who are advanced in the study will enjoy the perusal of an original work by a master in the science, who is always careful to distinguish between that which is certain and that which is possible, or more or less probable. With a judgment sound and critical, and in a style limpid and graceful, Professor Newcomb reviews the astronomy of past ages, and lays before his readers all that is known at the present time, with the principal theories on still doubtful points. There is a dewy freshness pervading the work, and the presence of a powerful mind is everywhere felt, yet the writer's self is never prominent; and even when some allusion to his own share in practical and theoretical departments would have been very natural, such allusion is not made, and we never meet with anything like the *quorum pars magna fui* of the pious *Æneid*.

For reasons which he assigns, Professor Newcomb considers that in

the great refractors of recent times, the limit of optical power for such instruments has been very nearly attained; and he thus discusses the relative advantages of refracting and reflecting telescopes. "If properly made and attended to, the refractor is easy to manage, convenient in use, and always in order for working with its full power. If its greatest defect, the secondary spectrum, cannot be diminished by skill, neither can it be increased by the want of skill on the part of the observer. So important is this certainty of operation, that far the greater part of the astronomical observations of the present century have been made with refractors, which have always proved themselves the best working instruments. Still, the defects arising from the secondary spectrum are inherent in the latter, and increase with the aperture of the glass to such an extent that no advantage can ever be gained by carrying the diameter of the lenses beyond a limit which may be somewhere between 30 and 36 inches. On the other hand, when we consider mere seeing-power, calculation at least gives the preference to the reflector. It is easy to compute that Lord Rosse's 'Leviathan,' and the 4-foot reflectors of Mr. Lassell, and of the Paris and Melbourne observatories, must collect from two to four times the light of the great Washington telescope. [26-in. aperture.] But when, instead of calculation, we enquire what difficult objects have actually been seen with the two classes of instruments, the result seems to indicate that the greatest refractor is equal in optical power to the great reflectors. No known object seen with the latter is too faint to be seen with the former. Why this discrepancy between the calculated powers of the great reflectors and their actual performance? The only causes we can find for it are imperfections in the figure and polish of the great mirrors. The great refractors are substantially perfect in their workmanship; the reflectors do not appear to be perfect, though what the imperfections may be, it is impossible to say with entire certainty. Whether the great telescope of the future shall belong to the one class or the other must depend upon whether the imperfections of the reflecting mirror can be completely overcome. Mr. Grubb, the maker of the great Melbourne telescope, thinks that he has completely succeeded in this, so as to insure a mirror of six, seven, or eight feet in diameter, that shall be as perfect as an object-glass. If he is right—and there is no mechanic whose opinion is entitled to greater confidence—then he has solved the problem in favour of the reflector, so far as optical power is concerned. But so large a telescope will be so difficult to manipulate, that we must still look to the refractor as the working instrument of the future, as well as of the past; though, for the discovery and examination of very faint objects, it may be found that the advantage will be on the side of the future great reflector." pp. 143–44.

The list of astronomical works, pp. 542–548, might, we think, be extended with advantage, so as to include more of both English and Continental treatises. On page 416, there is an obvious erratum of Arcturus for Antares. In the table of revolutions of binary star-systems, p. 439, ξ Herculis, and ξ Cancri should be ζ Herculis, and ζ Cancri, and while η Coronæ has a period of forty years assigned to it, η Coronæ Borealis is marked with sixty-seven years. It would seem that this last is a misprint for some other star.

We rejoice at the publication by an American astronomer of what, unless we are greatly mistaken, will rank as one of the best popular treatises on the science in the English language. Interested and instructed as we have been by its perusal, it deserves more than to be read for passing gratification; and we should not like to be without it as a work for continual reference.

American Journal of Mathematics, pure and applied. Editor-in-chief, J. J. Sylvester, LL.D., F.R.S., Cor. Mem. Inst. of France. Associate Editor-in-charge, William E. Story, Ph. D. (Leipsic). With the co-operation of Benjamin Peirce, LL.D., F.R.S., Professor of Mathematics in Harvard University, in Mechanics; Simon Newcomb, LL.D., F.R.S., Corr. Mem. Inst. of France, Superintendent of the American Ephemeris, in Astronomy, and H. A. Rowland, C.E. in Physics. Published under the auspices of the Johns Hopkins University. Volume I, Number I. Baltimore. Printed for the editors by John Murphy & Co. Trübner & Co., London. 1878. pp. 104.

Foreign, as well as American, mathematicians of eminence have engaged to support this journal, which will be published in volumes of about 380 quarto pages, each volume appearing in four numbers . . . as nearly as possible at intervals of three months. The subscription price of this journal is \$5 a volume, and single numbers may be obtained at \$1 50c. Communications may be addressed to William E. Storey, Johns Hopkins University, Baltimore, M.D. Subscriptions received in London by Trübner & Co. A list of the first 100 subscribers is given, and the contents of this first number are as follows:—

Note on a class of transformations which surfaces may undergo in space of more than three dimensions. By Simon Newcomb.—*Researches in the lunar theory*, I. By G. W. Hill.—*The theorem of three moments*. By Henry T. Eddy.—*Solution of the irreducible case*. By Guido Weichold.—*Desiderata and suggestions*. By Professor Cayley, No. 1. *The theory of groups*. *Note on the theory of electric absorption*. By H. A. Rowland.—*Esposizione del metodo dei minimi quadrati*. Per Annibale Ferrero, Firenze, 1876. By Charles S. Peirce.—*On the application of the new atomic theory to the graphical representation of the invariants and co-variants of binary quantics*. With appendices and plate. By J. J. Sylvester.

We can only briefly refer to one or two of the above papers. In his introductory remarks on the lunar theory, Mr. Hill says, "When we consider how we may best contribute to the advancement of this much-treated subject, we cannot fail to notice that the great majority of writers on it have had before them, as their ultimate aim, the construction of tables: that is, they have viewed the problem from the stand-point of practical astronomy rather than of mathematics. It is on this account that we find such a restricted choice of variables to express the position of the moon, and of parameters, in terms of which to express the co-efficients of the periodic terms. Again, their object compelling them to go over the whole field, they have neglected to notice many minor points of great interest to the mathematician, simply because the knowledge of them was unnecessary to the formation of tables. But the developments having now been carried extremely far, without completely satisfying all desires, one is led to ask whether such modifications cannot be made in the processes of integration, and such co-ordinates and parameters adopted, that a much nearer approach may be had to the law of the series, and at the same time their convergence augmented." The author explains his reasons for substituting rectangular for polar co-ordinates. He also advocates making a change in the received parameter in literal developments, and not making use of the same parameter throughout, but employing one in one class of inequalities, another in another, as may be most advantageous. He has preferred to use the differential equations in terms of the co-ordinates, rather than those in terms of the elements, and by augmenting the order of the equations he diminishes the number of unknown quantities and equations; in this

way avoiding the labour of making a preliminary development of R in terms of the elliptic elements. "Although," he says, "Delaunay's method is very elegant, and, perhaps as short as any, when one wishes to go over the whole ground of the lunar theory; it is subject to some disadvantages when the attention is restricted to a certain class of lunar inequalities." "In the present memoir I propose dividing the periodic developments of the lunar co-ordinates into classes of terms, after the manner of Euler in his last lunar theory,* to treat the following five classes of inequalities: 1. Those which depend only on the ratio of the mean motions of the sun and moon. 2. Those which are proportional to the lunar excentricity. 3. Those which are proportional to the sine of the lunar inclination. 4. Those which are proportional to the solar excentricity. 5. Those which are proportional to the solar parallax. A general method will also be given by which these investigations may be extended so as to cover the whole ground of the lunar theory. My methods have the advantage, which is not possessed by Delaunay's, that they adapt themselves equally to a special numerical computation of the co-efficients, or to a general literal development. The application of both procedures will be given in each of the just mentioned five classes of inequalities, so that it will be possible to compare them."

The exposition of the method of least squares. By Colonel Ferrero, Chief of the Geodetical Division of the Italian Survey. Reviewed and explained by Charles S. Pierce, is another article of astronomical interest. The writer thinks that the subject is here for the first time set upon its true and simple basis, and he recommends the treatise of Colonel Ferrero to those desirous of having a thorough practical acquaintance with the method, as decidedly the best and clearest on the subject.

This new journal intended primarily to supply a want, as a medium of communication between American mathematicians, and the publication of original investigations, is also to include critical and biographical notices and reviews of the most important recent mathematical publications, American and foreign. Its quarto form affords special advantage for the clear and symmetrical presentation of equations, &c. We have never seen a mathematical work so delightfully printed. It may, indeed, tax the student's brains, but it spares his eyes; which can by no means be always said of similar works.

Uniform Non-Local Time (Terrestrial time). A memoir by Sandford Fleming, C.M.G., M. Inst. C.E., F.G.S., F.R.G.S., Engineer-in-chief Canadian Pacific Railway, &c. pp. 32 (no date or publisher). Communications on the subject of the paper, may be addressed to the author, at the Canadian Agency, 31, Queen Victoria Street, London, E.C.

To obviate the growing inconvenience of varying local time in all parts of the world, the author proposes to divide the day into twenty-four parts, each marked with a letter of the alphabet to correspond with certain known meridians of longitude, so that the hour hand of a watch shall point in succession to each of the twenty-four divisions as it became noon at the corresponding meridian. When the hands of any one time-piece point, for example, to A or to G, the hands of every other clock or watch throughout the globe point to A or to G at the same moment. It is not proposed to do away with local time, but that each time-piece shall indicate terrestrial or universal time, together with local time. Various

* *Theoria Motuum Lunæ, nova methodo pertractata: Petropoli, 1772.*

plans are suggested for utilising existing timepieces, simply by furnishing them with new dial plates. One arrangement is to have the Roman numeral for local time inscribed on a movable disk which would admit of adjustment for any longitude without in the least disturbing the machinery of the instrument or interfering with the index hands. Only in the case of persons travelling beyond any particular local time district, would the local time disk require to be changed. The disk would then have to be moved until 12 o'clock noon coincides with the meridional letter of the new locality. The distinction between terrestrial time and local time would always be perfect; the former would invariably be known by letters, the latter as at present by the Roman numerals. Each of the twenty-four lettered meridians should be taken as standard longitudes for establishing approximate local time, and as a general rule all places should adopt the local time of the nearest of these meridians. For illustration, one of the lettered meridians, G, is assumed to pass through Greenwich, and terrestrial time for the moment to be G 45, then the approximate local time at other places around the globe would simultaneously be as exhibited in twenty-four plates of the watch face which follow, in which 12 o'clock noon is successively brought in conjunction with the letters which represent the twenty-four meridians: the hands and the dial for terrestrial time remaining constantly in the same relative position. Thus for Calcutta, G 45 corresponds with 6.45 p.m. For Washington it corresponds with 7.45 a.m., &c. Mr. Fleming thinks that clocks and watches now in use might, in a very inexpensive way, be so adapted as to show terrestrial in addition to local time; and in conclusion remarks that not the least important object which his scheme has in view, is to extend to the world similar advantages to those which have been conferred on England by the general adoption of Greenwich time, since the commencement of the railway era. His pamphlet seems to us deserving of attention.

The Stars in their Courses: a twofold series of maps, with a catalogue, showing how to identify, at any time of the year, all stars down to 5.6 mag. inclusive of Heis, which are clearly visible in English latitudes. By Thomas Sebastian Bazley, M.A., author of "Notes on the Epicycloidal Cutting Frame," and an "Index to the Geometric Chuck." Atlas folio, pp. 46, and 24 folding plates. Cloth, price, 15s. London: Trübner & Co., Ludgate Hill. 1878.

"The charts which are now offered as a help to a more familiar acquaintance with 'the stars in their courses,' are on a similar principle to those of Mr. Proctor's 'Constellation Seasons.' Besides, however, being on a larger scale, and containing many more stars,—all in fact which are distinctly visible in these latitudes—they are presented in duplicate. The first series numbered 1, 3, 5, &c., shows the stars by themselves as black disks on a white ground, without names, lines or letters. The second series, numbered 2, 4, 6, &c., shows 'the same stars in the same respective positions on corresponding maps, and also as black disks on white paper; but the boundaries of the constellations, the twenty-four hour circles of R. A., and Dec. circles at intervals of 10° are now introduced.' . . . The Milky Way is not shown: the maps are less confused without it . . . and it can be best studied with the aid of a celestial atlas—such as the library edition of Proctor, or the 'Modern Himmels' of Heis. The centre of each map corresponds with the zenith of the observer; and as a convenient means of referring any star overhead to the points of the compass on the horizon, and also of

finding its approximate altitude at the time for which each map is drawn: a lithograph, showing circles of altitude and radii of azimuth, is supplied, on thin paper and in triplicate, with each copy of this book. It should not be forgotten that distortion exists, and allowances must be made for it; still no group of stars, even at the most unfavourable portions of the map, is distorted beyond recognition," pp. 6, 7. The construction of the maps on the equidistant projection by mechanical means is described, and we are told that "all stars estimated by Heis as of not less than 5.6 magnitude were added to those extracted from the B. A. C.—irrespective of their magnitudes as assigned in the latter. But no star, though transferred to the present catalogue from the B. A. C. is figured in the maps if rated lower by Heis in the scale of magnitudes than 5.6. This numerical expression is not a decimal, but signifies that a star so designated is between the fifth and sixth magnitudes, and nearer the fifth than the sixth." . . . "The catalogue of Heis, which comprises all stars visible in Mid-Europe to the naked eye, extends to two-thirds of a magnitude lower than the limit adopted for the present maps, viz., to 6.7 mag.' His eyesight, however, must have been far above the average. In the preface to the catalogue of the "Atlas Celestis Novus," he says that the stars appear to him simply as luminous points divested of rays; that he can separate α Capricorni, ω Scorpii, δ and ϵ Lyrae, by unaided vision in a clear atmosphere, and that besides the six Pleiades, 'which are perceived by every ordinary eye,' he can also observe four others. Though similar instances of acute vision have been recorded, there are not many persons equally gifted; and it is probable that the above limit of 5.6 mag. will include all stars which, except on specially favourable nights, can be clearly and separately distinguished by ordinary vision." p. 9.

The assumed epoch is 1890. In the catalogue the constellations are enumerated alphabetically; and an asterisk is affixed to the stars which possess telescopic interest, and are within the reach of moderate instruments, as found in Webb's "Celestial Objects," &c. The maps are designed to represent the appearance of the heavens at intervals of two hours, and full instructions are given for their use. The total number of stars is 1,400. Mr. Proctor's "Constellation Seasons" (some time out of print) were not known to the author till after his own attempt had made considerable progress. "The magnitudes ascribed to Mr. Proctor," he says, "are taken from his Library Atlas—a work to whose marvellous accuracy and completeness I am able to offer the testimony of practical examination—perhaps without doing sufficient justice to the small differences in size of the disks which he has allotted to stars of similar denomination." Mr. Bazley, who dedicates this work to his children, concludes his detailed preface with the expression of the hope that these maps may be a useful preliminary to an atlas, and perhaps be welcome to observers of meteoric showers. The planispheres are $14\frac{1}{2}$ inches in diameter. The volume $17\frac{1}{2}$ by $11\frac{1}{2}$ inches, is elegantly and strongly bound in blue cloth, with gold and black lines. We think it will be highly prized by youthful students of the heavens, and likewise be acceptable to the amateur. It has evidently had much pains bestowed upon it. The maps of stars with no lines or letters are clearness itself, and the key-maps corresponding will enable anyone however unfamiliar with the heavens to identify constellations and stars without difficulty. We do not know if it would be practicable, without superseding the present form, to have also another in which the maps might be loose and unfolded in a common portfolio, and the letterpress thrown into a small companion volume. The price is decidedly moderate, and the execution by the Messrs. Spottiswoode leaves nothing to be desired.

Annals of the Astronomical Observatory of Harvard College. Vol. IX.

Observations made under the direction of the late Joseph Winlock, A.M., Phillips Professor of Astronomy and Director of the Observatory. Photometric Researches. Printed from the Sturgis Fund. Leipzig: Wilhelm Engelmann. 1878. Pp. vi. and 181, with plates.

Mr. C. S. Pierce, the immediate author of this investigation, is an assistant in the United States Coast Survey. In part of the work he was assisted by Mr. Henry Farquhar, of the Coast Survey Service. The whole was subsequently submitted to and accepted by the late Professor Winlock, and re-written and improved since his death. Ch. I. is on the sensation of light. II. on the number of stars of different degrees of brightness, with tables and formulæ; notices of the various catalogues of stars whose magnitudes have been reduced; these are the *Durchmusterung*, Argelander's *Uranometria Nova*, Heis's *Atlas Celestis Novus* (with a very long list of corrigenda), Ptolemy, Sâfi, Ulugh Beg, Tycho Brahe, Hevelius, Sir W. Herschel, Zöllner, Sir John Herschel, Seidel, Behrmann. The principle is laid down that in reforming the scale of magnitudes by photometry, it should be so adjusted that equal numerical differences of magnitude shall precisely represent equal ratios of light. pp. 1-88. Ch. III. contains original observations. These were made with a Zöllner's Astrophotometer "an artificial star is thrown into the field of a telescope, and its brightness is reduced by the rotation of a Nicol prism, until it matches in brightness any real star which is in the field at the same time. The Nicol prism being furnished with a graduated circle, the ratio of the reduction of the light is calculated from the reading. A third Nicol with an interposed quartz-plate, cut perpendicular to its axis, makes it possible to alter the colour of the artificial star, and this third Nicol is also furnished with a graduated circle. The flame which produces the artificial star is that of a kerosene lamp. Catalogue of 494 stars observed by the author with the magnitudes of various authorities [chiefly 1st to 5th mag.]. Remarks about known or suspected variables, pp. 88-141. Ch. IV.—Comparisons of the different observers; and table of the final corrections to the author's magnitudes in the above-named catalogue. On the general variability of stars, pp. 141-173. Ch. V. is on the form of the Galactic cluster, in which it is attempted to show the general forms of the surfaces of equal star-density throughout the cluster. These surfaces resemble Cassinian ovals in their sections, and are exhibited in plate III., which is a mean axial section of the Galactic cluster to the mean distance of a 6th mag. star. There appears to be a concentrated ring, lying much within the Milky Way, and at a distance of a $2\frac{1}{2}$ mag. star . . . the existence of a ring of bright stars, in the approximate plane of the Milky Way, has already been noticed by more than one astronomer. In regard to our position with reference to this ring, it is evident from the total number of stars on the Berenician and Magellanic sides, that we are upon the Berenician side of the most concentrated plane of bright stars, just as we are upon the same side of the Milky Way," p. 179. [The Berenician pole, spelled Beronician by Mr. Pierce, is that pole of the Milky Way which lies in Coma Berenices. He terms the opposite pole the Magellanic because the Magellanic clouds are upon that side.]

It will be remembered that Photometry is the special department selected for the great equatorial of H. C. No adequate appreciation can be formed of the merit of these Photometrical researches by Mr. Pierce, without examining the volume itself, of which we have endeavoured to give a general idea: and those who can do so will not need our commendation of it. Nevertheless, we cannot conclude this very defective

notice without the expression of our admiration for the patient toil to which each page testifies; the judgment displayed in the use of already existing material, ancient and modern; and the careful series of delicate observations in a field comparatively unworked—observations as skilfully made as they have been skilfully treated and reduced.

De l'Origine et de l'établissement des Mouvements Astronomiques. Par M. C. Lagrange, ancien élève à l'école militaire de Bruxelles. Bruxelles: F. Hayez, Imprimeur de l'Académie Royale de Belgique. 1878. pp. 48, with diagrams.

The author remarks that whilst the system of Vortices of Descartes was erroneous, he yet opened a new way by showing the gradual formation of globes by the condensation of matter at the centre of each vortex; so that his system has not been useless to science. The theory of Buffon, which imagined a comet to strike the sun outside of its centre of inertia, made the entire solar system the result of an accident. Laplace, who refuted this theory, put forth his well-known nebular hypothesis, which, unfortunately, is incomplete because it does not explain, but assumes, the rotation of the sun itself. After alluding to the meritorious, but defective, theory of R. Brück, M. Lagrange considers the possible continuous rotation of a yielding (*déformable*) mass subject to the attraction of another material system; and proceeds to determine the axis and movement of rotation in the case of the attracting system being at a great distance, both when the yielding system has a fixed centre, and when free to move in space. Next the cases are considered: 1. When *both* masses are free, and 2, when the yielding mass is not continuous. After discussing the variations in form of yielding masses, and establishing the existence of their rotation by the action alone of reciprocal attraction, it remains to examine the influence of rotation thus produced on the movements of the attracting systems. The first effect is to modify the form of the rotating mass, and that modified form subsequently influences the trajectories of the masses subject to its attraction. The trajectory of a material point subject to the attraction of a rotating mass is found to be a spiral or a conic, according as the deflecting force is feeble, or becomes nothing at the end of a certain time. Finally, it is shown that a yielding mass under the attraction of another such mass in rotation, takes a rotation in the same direction.

The nebular hypothesis of Laplace (preceded by a few years by that of Kant) may ultimately, with some modifications and additions, acquire a still greater amount of probability. As far as we are competent to judge, but without having yet endeavoured to follow this analytical investigation, which is one of a high order, step by step, M. Lagrange has been successful in disposing of one feature of incompleteness, and we shall be interested to learn the opinions of leading mathematicians in this country and elsewhere about his work. The second part will endeavour to show that reciprocal attraction has originated such yielding systems as have been here assumed, and will treat of the origin and establishment of the revolutions and rotations of globes.

One of the most recent and able writers,* who has reviewed the famous hypothesis of Laplace, after noticing its difficulties, concludes thus: "These difficulties may not be insurmountable. The greatest of them, perhaps, is to show how a ring of vapour surrounding the sun

* Professor Simon Newcomb. See also Sir Edmund Beckett's "Astronomy Without Mathematics."

could condense into a single planet encircled by satellites. The conditions under which such a result is possible require to be investigated mathematically. At the present time we can only say that the nebular hypothesis is indicated by the general tendencies of the laws of nature; that it has not been proved to be inconsistent with any fact; that it is almost a necessary consequence of the only theory by which we can account for the origin and conservation of the sun's heat; but that it rests on the assumption that this conservation is to be explained by the laws of nature as we now see them in operation. Should anyone be sceptical as to the sufficiency of these laws to account for the present state of things, science can furnish no evidence strong enough to overthrow his doubts, until the sun shall be found growing smaller by actual measurement, or the nebulae be actually seen to condense into stars and systems."

Instructions for observing the Total Solar Eclipse of July 29th, 1878. Issued by the U. S. Naval Observatory. Washington: Government Printing Office. 1878. pp. 30.

These instructions were drawn up by direction of Rear-Admiral Rodgers, U.S.N. Superintendent, by Prof. William Harkness, United States Navy. They are divided into nine sections: 1. Observations with the naked eye. 2. To photographers. 3. The photoheliograph. 4. The equatorial camera. 5. Telescopic observations. 6. Spectroscopic observations. 7. Polariscope observations. 8. Photometric observations. 9. Thermo-electric observations. Of these sections 1, 2, 5 and 8 describe such observations as can be made with the apparatus usually possessed by amateurs; the others relate to the powerful appliances and practical skill of professional astronomers and physicists. It is surprising how much can be done by a careful observer with no instruments at all. These instructions are written with admirable fulness and clearness. (One of the plates is a chart of the planets and stars in the vicinity of the sun on July 29th, 1878.) They must have conducted to the very successful observations which were made of this memorable eclipse, and will turn to account, we hope, again on future occasions, the next being May 17th, 1882, which will be best seen, it appears, in Upper Egypt and Arabia. But the greatest duration of totality will be only two minutes.

Star-gazing: Past and Present. By J. Norman Lockyer, F.R.S., Correspondent of the Institute of France. Expanded from shorthand notes of a course of Royal Institution Lectures, with the assistance of G. M. Seabroke, F.R.A.S. Macmillan & Co. 1878. pp. 496. 21s.

This work is divided into six books, as follows: I. The pre-telescopic age. II. The telescope. III. Time and space measurers. IV. Modern meridional observations. V. The equatorial. VI. Astronomical physics. This last including the determination of the light and heat of the stars, the chemistry of the stars, and spectrum analysis, celestial photography. The frontispiece is an autotype of Mr. Newall's 25-inch refractor. "Star-gazing," though an appropriate title, hardly conveys an idea of the character of Mr. Lockyer's work, which in fact covers ground not previously occupied by any English book we know of. Instruments are largely treated of, and illustrations given with unsparing profusion. Amongst these (217 in number) are many quite new; for instance the portrait of Tycho Brahe, from an original painting, his mural quadrant, details of the

ring of Saturn observed by Tronvelot with the 26-inch Washington refractor. (In some of the ordinary optical illustrations we have the usual lackadaisical-looking gentlemen whom it has been the fashion to introduce. These might well be replaced by an eye or a hand, as is elsewhere done). We think this will be found a valuable addition to our astronomical books; the information it gives will often not easily be found elsewhere, and a good index increases its utility. No doubt the author will be enabled in future editions to make improvements, correct inaccuracies which may be found, and render explanations more perspicuous. As examples of interesting pieces of information, we copy the following about the grinding and polishing of lenses and specula: "In the grinding of the two disks for Mr. Newall's telescope 1560 hours were consumed, the thickness of the crown disk having been reduced one inch in the process. In the case of specula, however, there is more to be done; and it is in this polishing of specula that the curve is altered from a circle to a parabola, by using a certain length of stroke, size of polisher, consistency of pitch, and numbers of other smaller matters, the proper proportionment of which constitutes the practical skill of the optician, and it is in accomplishing this that the finest niceties of manipulation come into play, and the utmost patience is required. 1170 hours were occupied in the grinding and polishing of the 4-ft. Melhonne speculum. This was equivalent to 2,050,000 strokes of the machine at 33 per minute for rough, and 24 for fine grinding. Dr. Robinson, in his description of the grinding operations, states that at the edge of one of the 4-ft. specula the distance of its parabola from the circle was only 0.000106 inch," pp. 128-9. And we learn that to produce a 7-in. mirror, Sir W. Herschel "would work continuously for 16 hours, his sister 'putting the victuals by hits into his mouth,'" p. 131.

In the last book we have some account of Dr. Janssen's recent labours at the new Physical Observatory established by the French government at Meudon. He makes use there of a photoheliograph, giving images of the sun on an enormous scale, plates having been produced of 15-in. diameter, showing details on the sun's surface, subtending an angle of less than one second of arc. And here we are reminded of a pleasing feature in the work we are noticing, the entire absence, that is to say, of the often irrepressible "ego." The reader, not otherwise aware of it, would not suppose from anything he here meets with, that the attention of Mr. Lockyer has been so successfully devoted to solar physics. One or two statements appear to us inaccurate. On page 32 the instrument of the Greeks is said to have read to 10', and those of Tycho to 10". Nominally, the latter may have been the case, but in reality we take it as moonshine. We may be pretty certain, as Tycho used only plain sights, that he could not make sure of anything approaching this accuracy, equivalent as it is to about the 180th part of the diameter of the sun or moon. γ Leonis is described (p. 352) as a yellow and green star. It never looks so to us. Smyth gives the colours bright orange and greenish yellow; Talmage, both yellowish, and again, straw-colour. Oxford University observations, 1878, deep yellow. Any one of these descriptions is much nearer the mark than "green." "Yellow and green" would much better suit γ Andromedæ. But these are small matters. Whilst not intended to be a treatise on practical astronomy, those who would have a good general idea of the appliances, ancient and modern, for the survey of the heavens, including the recent lines of investigation, which are so full of promise, will be grateful for this joint production of Messrs. Lockyer and Seashrope, and to such we cordially commend it as alike entertaining and instructive. Printing, paper, and illustrations are all very excellent.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

JUPITER.

Sir,—The equatorial zone of the planet is at present breaking up into singular alternations of light and shade. Last year this band presented, nestled closely up to the southern belt, several latitudinally elongated bright spots, at times followed by darkish black shadings. In September two of these were noticed to be extending in a N.W. direction, across the bright band. On July 27th in the present year, I saw a bright spot in the band, longitude 172° , extending almost from belt to belt; whilst in August other observers detected similar spots on either side of it. During September I saw Jupiter pretty well on a few occasions with a very fine $5\frac{1}{4}$ -inch Calver mirror, power 168, and found the band in places broken up into "port holes" and bright angular spots, separated by darker bands, some of which made an angle of only about 25° with the equator, and extended from S.W. to N.E., a position which may account possibly for the apparent changes noticed by Mr. Slack on July 31st. My $2\frac{1}{4}$ -in. Browning achr., power 115, showed me these inclined bands in the latter part of August.

With the $5\frac{1}{4}$ -in., on Sept. 6th, 9h. 45m., 13th 9h. 30m., 15th 9h., and 23rd 8h. 30m., I saw a fine loop belt thrown off from the equatorial side of the northern belt, some 30° in length, and about bisected by the 140^{th} degree of longitude. Independent of this, Sept. 6 and 23, a fine line belt was seen in the torrid zone joined to the northern belt at intervals by equally fine oblique belts, having a direction from N.W. to S.E.

In this part of the zone on Sept. 6, only a single bright spot is shown, near (E. of) the central meridian, and a brightish patch is just passing round the W. limb. On Sept. 23 the spot is again shown rather W. of the centre, whilst between it and the limb there is a large "port-hole." To the E. of it an oblique bright band crosses the zone from S.E. to N.W., and again E. of that there is another "port-hole." I must not say more now or I fear I shall make this long letter too long.

Southampton: Oct. 5.

FRANK C. DENNETT.

COMETS AND METEOR RADIANT-POINTS.

Sir,—I see by the catalogue of cometary radiant-points, given in the May number of the *Astronomical Register*, that the orbit of Comet I., 1870, intersects the earth's orbit at the point passed by the earth on August 12. I do not know whether this fact was known soon after the appearance of the comet, but it is unfortunate that it was not made known, and that a watch was not kept by astronomers generally on the succeeding apparition of the Perseids, to see if any meteors could be recognised as belonging to a shower connected with that comet. Its radiant point is given as R.A. $43^{\circ}5$, N.D. 53° , or very near that of the great comet of 1862 and of the Perseids. It would have been very interesting to have ascertained

(if possible) whether the comet of 1870 made any difference to the shower in neighbouring years. No effect seems to have been perceived, but perhaps if it had been looked out for it might have been. I commend to the British Association Committee on Luminous Meteors the desirability of having the radiant-points of comets calculated soon after they appear, in all cases where their orbits come near the earth's, and having a watch kept for any meteors that may be connected with them.

Sunderland: Sept. 27, 1878.

T. W. BACKHOUSE.

DOES THE GASEOUS PART OF THE CORONA VARY WITH SUN SPOTS?

(From the *Athenæum*.)

Besides the observation of the 1474 line by Prof. Young, which was quoted in my letter of last week, I am informed that Prof. Eastman, making use of a spectroscope provided with a slit during the recent eclipse, traced the 1474 line to a distance of from 10' to 20' from the moon's limb, and that he carried his slit twice around the sun without losing sight of the line.

It will be noticed that Prof. Respighi is the only observer who has seen the monochromatic images of the *corona* without a slit. The rings seen by Mr. Lockyer in 1871 corresponded to the area of the chromosphere and not to that of the corona, for he speaks of them, in his description in *Nature*, vol. v. p. 218, as "certainly not more than 2' high." His observation was made through five prisms of 45° each, by which the light of the corona must have been very greatly reduced.

No one, as far as I am aware, during the recent eclipse used a prism of small angle placed in front of the object-glass similar to the prism with which Prof. Respighi's observation was made in 1871. The proportion of light lost by reflection at the surfaces of such a prism would be much less than the proportion lost by reflection at the surfaces of a 60° prism, such as was used by Prof. Rockwood and others during the recent eclipse, and would be considerably less than the proportion of light lost in transmission through a direct vision prism, similar to the one made use of by myself. The evidence, therefore, as to the monochromatic images of the corona being fainter on the present occasion than in 1871 is not perfectly satisfactory.

Dr. Magrini, during the eclipse of 1842, and Mr. Dunkin, during the eclipse of 1851, observed the corona through glass prisms without slits, but neither of them noticed the monochromatic images. I am not inclined to attach much weight to such negative evidence, but the fact should be borne in mind in speaking of the negative results obtained during the recent eclipse as remarkable.

If the height to which the bright line spectrum of the corona has been traced above the sun's limb bears any direct relation to the intensity of the bright lines, the brightness of the gaseous part of the corona does not vary with sun-spots; for in 1870 Winlock, Young, and Harkness traced the 1474 line to heights of 25', 16', and 15' respectively. During the eclipse of 1871 Janssen traced the 1474 line and the hydrogen spectrum to a height of 10' (about); Herschel traced the 1474 line to a height of 20' (at least); and Moseley traced the 1474 line to a height of 22' (about); while in April, 1874, when the number of sun-spots had greatly decreased, Stone with an instrument not very well suited to the purpose, the only one which he had at his command at the Cape, traced the 1474 line to more than "1° from the sun's centre," in other words, to 45' or more from the sun's limb.

I have not referred to the spectroscopic observations made during the eclipses of 1868 and 1869, as attention was not directed during these earlier eclipses to determining the distance from the limb to which the bright lines could be traced, but it is worthy of note that in 1868 Pogson, Tennant, and Rziha all describe the corona as giving a continuous spectrum without bright lines, and Rayet describes the bright lines of the coronal spectrum as fading out at a height of only about 7'. In 1869 Harkness, judging from a diagram he gives, saw the green line when the centre of his slit was at a height of about 5', and it would seem that he could not trace it to a greater altitude. I am not aware, however, that there is any evidence which directly goes to prove that the green line was not visible during the eclipse of 1869 at a greater altitude than 5'.

It is possible that the brightness of the monochromatic images of the corona may vary greatly from month to month, or even from day to day, and that the gaseous part of the corona, as observed on any particular day, may not at all correspond with the general law of variation. We know that sun-spots vary greatly from month to month, and that the spots observed on any particular day could not be taken as indicating the period of sun-spot development. It would, therefore, be very rash to found any sweeping theory on the few observations of the coronal spectrum which we at present possess; but as far as those observations go they tend to show that the law which some theorists have too precipitately urged does not hold.

A. C. RANYARD.

ENCKE'S COMET.

Writing under date of Aug. 13, 1878, Mr. J. Tebbutt, of Windsor, N.S.W., announces the re-discovery by himself on Aug. 3 of Encke's comet, in nearly the position assigned by Dr. von Asten's ephemeris, published in *A. N.* 2197. We learn with regret the death of Dr. von Asten at the early age of 36.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN NOVEMBER, 1878.

BY W. R. BIRT, F.R.A.S., P.S.S.

MARE CRISIUM. Thirteen years have elapsed since the surface of this well-known formation was most carefully surveyed by Messrs. Dawes, Knott, Birt, Bird, and Ingall, and the result of the survey with apertures varying from 5 inches (Dialyte) to 12 inches? silvered glass reflector—the instruments employed by Messrs. Dawes and Knott were achromatics, both over 6 inches—including 49 craters were published in the volume of Reports of the British Association for the Advancement of Science for the year 1865, pp. 292, 293. As these objects were well observed in 1864 and 1865 the appearance of Schmidt's large map affords a good opportunity of ascertaining if they had been noticed by Schmidt in the course of his observations for constructing his map. From a careful comparison of the map with the original drawings of the before-mentioned observers, especially those by Dawes and Knott, it is found that of the 49 objects published in the Report of the Association, 24 are not in Schmidt's map. The catalogue published in the Report of the Association is arranged in accordance with the principle mentioned in pp. 286 to 291.

The following table will show the distribution of these craters over the Mare Crisium.

Zone. III. Lat. 10° to 15° long.		IC ν 60° to 65° 1	IC μ 55° to 60° 7	IC λ 50° to 55° 2	= 10
V. Lat. 15° to 20° long.	IC τ 65° to 70° 2	IC σ 60° to 65° 5	IC ρ 55° to 60° 1	IC π 50° to 55° 5	= 13

with one in area IC ψ , latitude 20° to 25°, longitude 60° to 65°, will make the 24 not in Schmidt's map.

On Beer and Mädler's map, which is divided by meridians of lunar longitude and parallels of lunar latitude, these areas are easily found; the objects depicted by B. and M. in each will sufficiently guide the observer in his examination; thus Picard is found in area IC λ , it is just east of the meridian of 55°; five degrees west of the meridian 55° constitutes the area IC μ . As the above and other omissions occur on Schmidt's map which is not divided, in the body of the map, by meridians and parallels, it would be well for observers in possession of large instruments and B. and M.'s map—the first edition is the best—carefully to examine the surface of the Mare Crisium area and note every crater and other object as it appears at sunrise and sunset, and also every *white spot* as it appears at the time of the full moon.

HYGINUS AND NEIGHBOURHOOD. Miss Ashley, of Bath, reports that of twenty-seven craters given by Schmidt in the neighbourhood of Hyginus, she saw in September under almost all illuminations five as well as the spiral mountain *quite plainly*. She also saw seven as white spots, and a group of three as a *single white spot*, which with Hyginus numbered sixteen as seen by her. Eleven given by Schmidt she failed to detect. Members of the Selenographical Society may obtain a perusal of Selenographical Papers, No. 12 (MS. to be returned in three days), by writing to W. R. Birt, inclosing stamp for transmission, at the address given below. The paper contains drawings and observations of the region with a tracing from Schmidt's map.

9, Crescent Grove, Stratford, London, E.

October 21, 1878.

We are sorry to announce the death of an amateur astronomer and occasional contributor to the *Astronomical Register*, Canon Gribble, who was twenty years chaplain to the British Embassy at Constantinople. He is said to have been an accomplished linguist and astronomer. He died of a malady contracted during the late troubles in the Turkish capital.

Books Received.—Observations and Orbits of the Satellites of Mars. By Asaph Hall. Government Press. Washington. 1878.—Annals of the Astronomical Observatory of Harvard College. Vol. IV. Part II. Cambridge: John Wilson. 1878.—The London Science Class Books. London: Longmans, Green & Co. 1878.

ASTRONOMICAL OCCURRENCES FOR NOVEMBER, 1878.

DATE.		Principal Occurrences.	Jupiter's Satellites.	Meridian Passage.	
		h. m.		h. m. s.	h. m.
Fri	1	9 50	☾ Moon's First Quarter Saturn's Ring : Major axis = 42".76 Minor axis = 1".47		Saturn. — 9 8.2
Sat	2		Sidereal Time at Mean Noon 14h. 46m. 30.23s.	2nd Ec. R. 5 56 25 1st Oc. D. 9 28	9 4.1
Sun	3		Sun's Meridian Passage 16m. 18.92s. before Mean Noon	3rd Tr. E. 5 44 1st Tr. I. 6 37 3rd Sh. I. 7 21 1st Sh. I. 7 55 1st Tr. E. 8 57	9 0.0
Mon	4			1st Ec. R. 7 32 36	8 55.9
Tues	5	8 0	Occultation of B.A.C. 8276 (6½)		
		9 20	Reappearance of ditto		
		10 56	Occultation of 22 Pis- cium (6)	1st Sh. E. 4 44	8 51.8
		11 58 14	Reappearance of ditto Conjunction of Moon and Saturn 6° 58' S.		
Wed	6				8 47.7
Thur	7			2nd Tr. I. 8 50	8 43.6
Fri	8				8 39.6
Sat	9	14 33	☉ Full Moon		
		3 46	Occultation of μ Arietis (5½)	2nd Ec. R. 8 34 21	8 35.5
		4 37	Reappearance of ditto		
		5 51	Near approach of 9 Tauri (6)		
Sun	10	9 31	Occultation of 17 Tauri (4)		
		10 46	Reappearance of ditto		
		9 47	Occultation of 16 Tauri (5½)		
		10 42	Reappearance of ditto		
		10 23	Occultation of 20 Tauri (5)		
		11 21	Reappearance of ditto	3rd Tr. I. 6 18	8 31.4
		10 33	Near approach of 19 Tauri (5)	1st Tr. I. 8 35	
		10 49	Near approach of 23 Tauri (5)		
		11 11	Occultation of η Tauri (3)		
		12 2	Reappearance of ditto		
		12 38	Near approach of 27 Tauri (4)		
		12 40	Near approach of 28 Tauri (5½)		
Mon	11			1st Oc. D. 5 53	8 27.4

DATE.	Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
	h.	m.	1st Sh. I. 1st Tr. E. 1st Sh. E.	h. m. s. 4 19 5 24 6 40	h. m. Saturn. — 8 23'4
Tues 12					
Wed 13	16 12	Occultation of α Gemini- norum (3½)			8 19'3
	17 21	Reappearance of ditto			
Thur 14	8 7	Occultation of γ Gemini- norum (6)	3rd Ec. R.	4 58 38	8 15'3
	8 42	Reappearance of ditto			
Fri 15	14 41	Occultation of B.A.C. 2854 (6)			
	15 13	Reappearance of ditto			8 11'2
		Illuminated portion of disc of Venus=0.997 Illuminated portion of disc of Mars=0.990			
Sat 16	10 14	Occultation of π^2 Cancri (6½)	2nd Oc. D.	5 49	8 7'2
	10 26	Reappearance of ditto			
Sun 17	5 58	☾ Moon's Last Quarter			8 3'2
	20 15	Near approach of 43 Leonis (6)			
Mon 18	14 40	Occultation of p^2 Leonis (6)	2nd Sh. E. 1st Oc. D.	6 17 7 53	7 59'2
	15 31	Reappearance of ditto	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	5 3 6 15 7 23 8 35	7 55'2
Tues 19		Sidereal Time at Mean Noon 15h. 39m. 28.80s.			
Wed 20		Sun's Meridian Passage 14m. 13.27s. before Mean Noon	1st Ec. R.	5 51 51	7 51'2
		Saturn's Ring: Major axis=41".54 Minor axis=1".26	3rd Oc. R. 3rd Ec. D.	4 15 5 27 56	7 47'2
Fri 22	10	Conjunction of Moon and Mars 5° 49' N.			7 43'2
Sat 23	21 10	● New Moon			7 39'2
	18	Conjunction of Mars and Venus 5° 14' N.	4th Ec. D.	5 39 7	7 35'3
Sun 24			2nd Sh. I. 2nd Tr. E.	5 56 6 33	7 31'3
Mon 25	4	Conjunction of Moon and Mercury 1° 17' N.	1st Tr. I. 1st Sh. I.	7 3 8 11	7 27'3
Tues 26	0	Uranus at quadrature with the Sun	1st Oc. D. 1st Ec. R.	4 22 7 47	7 23'4
Wed 27			3rd Oc. D. 1st Sh. E.	4 56 5 0	7 19'5
Thur 28	8	Conjunction of Moon and Jupiter 0° 31' S.			7 15'5
Fri 29	10	Conjunction of Mars and α Libræ 0° 4' S.			7 11'6
Sat 30	8 8	Near approach of B.A.C. 7697 (6½)			
DEC. Sun 1	4 37	☾ Moon's First Quarter			7 7'6

THE PLANETS FOR NOVEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ' "		h. m.
Mercury ...	1st	14 44 40	S. 16 14	4".6	0 2.1
	9th	15 34 38	S. 20 26	4".8	0 20.5
	17th	16 25 34	S. 23 33½	5".0	0 39.8
	25th	17 16 56	S. 25 26	5".4	0 59.6
Venus ...	1st	13 59 47	S. 10 57	9".8	23 13.4
	9th	14 38 26	S. 14 26½	9".8	23 20.5
	17th	15 18 16	S. 17 32½	9".8	23 28.8
	25th	15 59 24	S. 20 8½	9".8	23 38.3
Jupiter ...	1st	20 8 14	S. 20 53	36".0	5 54.8
	9th	20 12 30	S. 20 39½	35".0	4 57.6
	17th	20 17 23	S. 20 24	34".4	4 31.0
	25th	20 22 49	S. 20 6	33".8	4 5.0
Saturn ...	1st	23 52 13	S. 3 34½	17".6	9 8.2
	9th	23 51 0	S. 3 41	17".0	8 35.5
	17th	23 50 10	S. 3 45	16".8	8 3.2
	25th	23 49 43	S. 3 46	16".6	7 31.3
Neptune ...	1st	2 26 23	N. 12 33½	...	11 41.9
	17th	2 24 41	N. 12 25	...	10 37.3

Mercury sets a few minutes after the sun on the 1st, the interval increasing to about half an hour by the end of the month.

Venus rises about three-quarters of an hour before sunrise at the beginning of the month, the interval decreasing.

Jupiter sets two hours and a half before midnight on the 1st, the interval increasing.

Saturn sets three hours after midnight on the 1st, the interval decreasing to about one hour on the 30th.

JUPITER.

1878. oh. Gr.	Angle of position of 2½'s axis.	Latitude of Earth Sun above 2½'s equator.		Equat. diameter.	Defect of illumi- nation.
Nov. 1	346°08	—0°60	—0°23	38°05	0°37
11	345°59	0°56	0°19	36°97	0°32
21	345°02	—0°52	—0°14	36°00	0°28

The assumed First Meridian passes the middle of the illuminated disk at the following times :

1878. G. M. T.

	h. m.		h. m.		h. m.
Nov. 1	1 30.8	Nov. 3	13 4.3	Nov. 6	0 37.8
	11 26.4		23 0.0		10 33.4
	21 21.9	4	8 55.4		20 29.0
2	7 17.5		18 51.0	7	6 24.6
	17 13.1	5	4 46.6		16 20.2
3	3 8.7		14 42.2	8	2 15.8

1878. Gr. M. Time.

Nov. 15	10.8	Tethys.	sp.
16	6.1	Dione	w.
	7.0	Rhea	np.
	8.1	Encel.	e.
	9.5	Tethys.	nf.
17	4.4	Titan.	[sf. limb.
	8.1	Tethys.	sp.
	8.4	Titan.	sf.
	12.1	Encel.	sf.
18	6.8	Tethys.	nf.
	9.5	Encel.	w.
	11.4	Dione.	np.
19	6.8	Encel.	nf.
	11.7	Rhea.	e.
	12.3	Dione.	sp.
20	5.8	Encel.	sf.
	8.7	Dione.	e.
	10.2	Rhea.	nf.
	10.8	Encel.	e.
	11.2	Tethys.	np.
21	5.1	Dione.	np.
	8.2	Encel.	sp.
	9.9	Tethys.	sf.
22	6.0	Dione.	sp.
	7.2	Encel.	np.
	8.5	Tethys.	np.
	12.1	Encel.	w.
23	7.2	Tethys.	sf.
	9.5	Encel.	nf.

1878. Gr. M. Time.

Nov. 24	5.8	Tethys.	np.
	8.5	Encel.	sf.
	11.2	Dione.	w.
25	5	Titan.	nf. limb.
	5.9	Encel.	w.
	7.6	Dione.	sf.
	7.8	Rhea.	np.
	10.8	Encel.	sp.
	11	Titan.	np. limb.
26	6.3	Rhea.	w.
	8.5	Dione.	nf.
	9.8	Encel.	np.
	10.9	Tethys.	w.
27	4.9	Dione.	w.
	7.2	Encel.	e.
	9.6	Tethys.	e.
28	8.2	Tethys.	w.
	11.2	Encel.	sf.
	12.5	Rhea.	e.
29	6.9	Tethys.	e.
	8.5	Encel.	w.
	10.1	Dione.	np.
	11.0	Rhea.	nf.
30	5.5	Tethys.	w.
	5.9	Encel.	nf.
	11.0	Dione.	sp.
Dec. 1	7.4	Dione.	e.
	9.9	Encel.	e.

The transits of Titan across the disk of Saturn on Nov. 9 and 25 occur at convenient hours, and ought to be carefully observed. They are the last observable ones for a dozen years to come. Observations of the times of the conjunctions with the limb, or of the times when the perpendicular line from Titan upon the axis of the ring is a tangent to the following or preceding limb of the ball, are practically more valuable than observations of the ingress or egress, though the latter ought not to be neglected. Iapetus reaches its superior conjunction, south of the planet, on Nov. 19.

A. M.

ASTRONOMICAL REGISTER.—Subscriptions received by the Editor.

To Dec., 1877.

Green, S.

To Sept., 1878.

Brothers, A.

Chichester, Sir Bruce.

Dale, E.

Lean, W. S.

Lee, G. H.

To Oct., 1878.

Calver, G.

Lelgh, J.

To Dec., 1878.

Elliott, E.

Lewis, H. K.

To Sept., 1879.

Hankey, H. A.

To Dec., 1879.

Birt, W. R.

To Feb., 1880.

Guyon, G. F.

The *Astronomical Register* is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings per Quarter, payable in advance**, by Penny postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, Angel Court, Throgmorton Street, E.C., not later than the 15th of the Month.

The Astronomical Register.

No. 192.

DECEMBER.

1878.

ROYAL ASTRONOMICAL SOCIETY.

Session 1878—79.

First Meeting after the Long Vacation, November 8th, 1878.

Lord Lindsay, M.P., F.R.S., &c., *President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

The Rev. J. M. Coates, The Goddards, Moulton, Spalding.

Mr. B. G. Jenkins, 4, Buccleuch Road, West Dulwich,
were balloted for and duly elected Fellows of the Society.

On the recommendation of the Council the following foreign
astronomers were elected Associates of the Society :

Prof. O. Bruhns, of the Observatory, Leipzig.

Baron H. von Dembowski, of Gallarate, Italy.

Mr. G. W. Hill, of New York.

Prof. E. Schönfeld, of the Observatory, Bonn.

Mr. Glaisher announced that 133 presents had been received since the last meeting. He drew special attention to a set of the *Sidereal Messenger*, which had been presented to the library by Mr. Burnham, of Chicago, and also to a copy of Schmidt's new map of the moon, which had been presented to the Society by the Prussian Government. The thanks of the Society were voted to the several donors.

Mr. David Gill read a paper *On the results of meridian observations of the Mars stars of comparison*. He said it was a paper suitable only for printing, and that he would merely give a brief *précis* of it to the meeting.

Before leaving England for Ascension he had applied to the principal observatories for co-operation, and he desired now to record the hearty response which had been made to his requests, and to tender his warmest thanks to the directors of the various

observatories for the valuable assistance they had rendered to him.

He had still to receive the results of a long series of observations made at the Dudley Observatory in 1877 and 1878, as well as the results of the observations made at Sydney, and at Liverpool in 1877. He had, however, received the results of observations made at

Greenwich.	Königsberg.
Oxford.	Leiden.
Paris.	Cordoba, Argentine Republic.
Berlin.	Cambridge, U.S.
Pulkowa.	Washington.
Leipzig.	Melbourne.

It was not often they had an opportunity of comparing together a large number of observations of the same stars, made at so many standard observatories at the same epoch. He was much interested in comparing the various results, and had entered into a thorough discussion of them, including a reduction of the old observations for proper motion. He had gone into details which could but be followed by those who chose to read the paper afterwards.

The most remarkable result which came out was this, that between these standard observatories very great discordance was found to exist in the absolute Right Ascension. Thus, for example, whilst Königsberg, Greenwich, and Pulkowa represented nearly the mean system of right ascension, the Washington right ascension required a correction of -0.120 sec., and the Oxford a correction of $+0.076$ sec. to reduce them to the same system. Hence a systematic difference of nearly 2-10ths of a second of time exists in the right ascensions of the two observatories. Thus, if a theorist were provided with observations of a minor planet made at these observatories he would find an unaccountable discordance of three seconds of arc in the two series. It became, then, an interesting as well as an important matter to account for these differences. After considerable examination of the matter, he came to the conclusion that the discordance might have three sources:

1. A systematic difference in the adopted places of the clock stars.
2. Errors having a mechanical origin from defects of the instrument, such as a changing error of collimation due to buckling of the cube, or a changing error of azimuth due to pivot error. (The time stars having in general a greater N. declination than the mass stars, such instrumental defect would affect the places of the zone systematically.)

3. An error depending on the magnitude, due to the habit of the observer in observing bright and faint stars; some observers having a tendency to observe bright stars too soon, and *vice versa*.

These conclusions were arrived at gradually. He had found that the greatest errors that could arise from the first cause did not amount to more than ± 0.03 sec.

Thus, for the few observatories whence he had secured the necessary data, a careful examination of the clock errors derived from clock stars of various declinations did not seem to show any errors due to the second cause which could amount to 0.02 sec. or 0.03 sec., and he had therefore been driven to the conclusion that they must arise from the third cause.

He had, therefore, addressed a circular to the various observatories requesting the observation of some bright stars near the Mars stars, together with re-observation of some of the Mars stars. From these data he hoped to be able to deduce valuable conclusions, and to determine not only the absolute differences of right ascension, but also arrive more accurately at the absolute right ascensions of the stars.

He was happy to state that at Leiden, Pulkowa, and Cambridge, U.S., the matter had been energetically taken up. From Leiden he had already received the results of some interesting experiments. A diaphragm having been applied in front of the object-glass, the transit of a star over five wires was observed, the diaphragm was then removed and the star observed over the last five wires, the transits being then reduced to the middle web, the difference due to personality depending on magnitude becomes known.

In order to avoid possible systematic change of collimation from the application of a diaphragm, the transits were taken alternately with the diaphragm used for the first set of webs and the last set of webs, but no difference was found in the results. The three observers at the Leiden Observatory were all, however, affected with personality depending on magnitudes in the same direction, viz :

$$\left. \begin{array}{l} \text{sec.} \\ -0.070 \\ -0.025 \\ -0.051 \end{array} \right\} \text{For a difference of } 2\frac{1}{2} \text{ mag.}$$

and when these corrections were applied to their observations, the systematic error of the Leiden R.A. nearly disappeared.

He had concluded, however, that the final discussion of R.A. could only be made after the results of all the special observations had been received. In the meanwhile he had discussed the observations on the supposition that the results of each observatory

are affected by a systematic error common to all the stars, by a probable error depending on the *square root* of the number of the observations, and by probable error peculiar to each star not affected by the number of observations. He had determined all these errors and thus obtained a system of weights by means of which the most probable place of each star was deduced. The discussion led to some interesting conclusions which could only be followed by a perusal of the paper in print.

Mr. Gill then said he had now to bring before the Society a report on the observations of Mars made at Ascension, and he did so in the form now laid before the Society at the suggestion of the Astronomer-Royal. A long time must elapse before he would be able to give the definitive result of the whole of the observations, because so many investigations have sprung out of the original one, investigations that required for their satisfactory discussion new observations, such as those to which he had already alluded. He was continually being asked by Fellows when the results of the expedition would be published, and he felt it was due to the Society to show that no time was being lost. He had, therefore, discussed the observations of Mars from Sept. 4th to 11th inclusive, and these formed the subject of the present paper.

He first showed how the true determinations were made, and how the latitude and longitude of the station had been arrived at, how the tabular quantities had been computed, and how the actual observations were made.

With regard to the latter he said the observations had been made in two ways :

A In which the star was placed in the centre of the planet.

B In which the star was placed alternately on opposite limbs of the planet.

In reply to a question by the Astronomer-Royal, he stated that the observations under discussion had all been made by the latter method, but that both before and after opposition many observations had been made by the other method. The same corrections for division error of the scale had been employed as determined in connection with the Juno investigation, but the errors of the screw had been re-determined by daylight observations on 28 days about the time of the Mars observations, thus the conditions of the screw as to wear were not changed between the time of determination and the time of employment of the screw in actual observation.

The speaker then went into the methods of calculation and the results.

He thought he had shown how desirable it was to start with star places of very great accuracy, and to regard them as

absolutely known in the final discussion. If this is not done many valuable observations must be lost, and the door is opened for an arbitrary solution of observations, and an arbitrary treatment of results which is most undesirable.

Employing the star places arrived at in the previous paper, and rejecting no complete observation whatever between Sept. 4 and Sept. 11, the probable error of one measure of the distance between a star and the centre of Mars is $\pm 0''.30$, and this includes both the error of the star's place and the error of the heliometric measurement. It would appear, then, that a single observation of Mars by the heliometer is as accurate as an average observation of contact on a transit of Venus, while the parallax factors are quite as favourable.

Thus a single observer on a fine night and at a suitable station can do as much towards the determination of the solar parallax as nine or ten costly transit of Venus expeditions, and his observations have moreover the enormous advantage that they are capable only of one interpretation.

Beyond the reasons already stated, which had induced him to lay the paper before the Society in its present form, there was the hope that now or at some future time he would have the benefit of criticism on the methods he proposed to employ in deducing the definitive result. There was only one form of criticism which was useless, and it was one unfortunately too common, viz., that the result was right or wrong according as it agreed with the critic's preconceived ideas as to what it ought to be (laughter).

It was for this reason that he had not published the assumed value of the parallax. All useful and honest criticism could equally well be made without this information, and therefore it was better to defer the publication of any result until the final result had been arrived at. (Applause)

A note on some remarks by Mr. Maxwell Hall was then read.

Sir G. B. Airy said the communication which has been made is a very important one; I think the point which Mr. Gill has indicated as to the possible difference in the personal equation in observing stars of different magnitude is likely to be important in questions of this kind. It is only within a few days that Mr. Gill was good enough to mention the subject to me, and I have not had any opportunity of verifying his suggestion at present, but, if possible, I will do so. with regard to some of those stars which are most interesting to Mr. Gill. In the next place I would observe that when I made a communication on this subject about twenty years ago, and spoke of various methods of determining the parallax of the sun, I referred to

the method of determining the difference of right ascension between Mars and stars east and west of the meridian as being what I considered the best. Now, at that time there had been no good instances of the use of the heliometer in England, and there were then known fewer of the small planets exterior to Mars, and none of them came so near to the orbit of Mars as some of them which have been recently discovered. Under the considerations which were then before my eyes, I was well justified, and perhaps should still be justified, in recommending the comparison of the positions of Mars with the neighbouring stars as the best method which could be recommended. But there is another consideration which suggested itself to me lately, which may possibly affect such observations. When you observe Mars and a star near the horizon you observe the planet and star under circumstances of atmospheric refraction and dispersion which (from difference of colour and other causes) may affect them differently. Supposing there is any such error, it will be seen that at rising and setting it would operate in the same direction with regard to the meridian, and so would affect the absolute determination of the parallax. Now, I think that the recommendation which Mr. Gill has made, that the oppositions of the minor planets should be observed, is very important.

Mr. Gill: I think Dr. Galle was the first to recommend the observation of the minor planets at opposition.

The Astronomer-Royal: I am glad to be corrected. I think that the recommendation is important, because we might expect that the atmospheric dispersion would produce about the same effect for the small planets and the stars with which they were compared. I think these are the principal points I have to remark upon. I would only add that I think Mr. Gill exercises a wise discretion in suspending anything like an official declaration until his reduction has been completed. (Applause.)

Mr. Penrose was called upon to read a paper on his observations of the total eclipse of the 29th of July last. His observing station was near to Denver, at an altitude of about 5,500 feet above the sea. His principal object was to draw the corona, and he also tried to note the time of the four phases, in which he partly succeeded, but did not get good observations of the first contact. As totality approached he was struck with the gradual darkening of the sky. He saw no sign of Bailey's beads, but immediately the light of the crescent was lost he caught sight of the corona, which he had endeavoured to represent in a large drawing which was shown at the meeting. Only the upper part of the drawing pretended to accuracy—the lower part had been sketched in a more hurried manner. He had also added some

outer rays, which had been observed by Prof. Abbé and other observers. These were given in fainter tints than the parts of the corona observed by Mr. Penrose himself. Prof. Abbé supposed the origin of these streamers to be meteoric. Mr. Penrose showed a diagram to illustrate this view, which was in accordance with his own observations. Mr. Penrose noticed that after the reappearance of the sun several serpentine lines were to be seen moving over the ground at a rate which he estimated to be about five miles an hour. In concluding he wished to add a word of thanks to the American astronomers as well as to the railway companies and the managers of the White Star Line of ships. (Applause.)

Dr. Schuster read a paper on his observations made during the same eclipse. He said that the observing station he chose was Las Animas, in South Colorado; he made use of a $4\frac{1}{2}$ -inch achromatic telescope and a spectroscope. Unfortunately he had an accident with the florescent cell which he intended to use, so that on the day of the eclipse he had to change his plans, and confine himself to observing the visible part of the spectrum. As the eclipse drew on he covered his left eye with a piece of black velvet, and all the observations that preceded the eclipse were made with his right eye. As a matter of experiment he looked at a sheet of ordinary white paper about half an hour before the eclipse, both with the covered and uncovered eyes, and while with the one eye it appeared a dull white to the other it was absolutely dazzling. He observed the spectrum as the eclipse proceeded and was greatly struck with the way in which the blue and violet regions came out as the eclipse proceeded. Afterwards he mentioned this to Professor Eastman, who was observing close by, and found that he had observed the same fact. Professor Young also said that just before totality he could see the lines further up towards O than before the eclipse. He believed that it was not accidental, but that as the eclipse went on part of the aqueous vapour condensed, and the atmosphere became more transparent to the violet rays. From ten minutes before the totality he set himself to observe whether the corona was visible before the totality, but it was close upon totality before he could make it out. He saw the corona shoot out on one side and he began to take up the beats of the chronometer; he counted six seconds, before the sun finally disappeared. When totality began he went to the spectroscope, and asked Mr. Haskins, who was directing the instrument for him, to put the finder on a bright part of the corona. The telescope of the spectroscope was pointed to the violet, and he was startled with the intensity of the light, and the spectrum was visible as far as wave length

4070. The slit was then moved to three-tenths of a diameter from the limb, but he could detect no difference in the intensity. He then moved the telescope of the spectroscope into the green region, but did not see any lines; but on looking a second time thought he saw two fine green lines; unfortunately, before he could put the finder on to them, the sun reappeared. Professor Eastman afterwards told him that he had seen the green lines. He saw no dark lines, nor were they seen by Professor Eastman or Professor Young. With regard to the polariscope observations, he thought that they were likely to give further information as to the reflection of light from the corona. From the observations made by Winter and others, it appeared that there was a place at a certain distance above the sun's limb where the corona was more strongly polarized than either above or below. If the question were examined theoretically it would be found the polarization ought to be gradually decreased as you went outwards [Here Dr. Schuster showed upon the board an expression for determining the amount of polarization which theoretically should be given by a small area within the corona], above the area of maximum polarization he had referred to the polarization decreased again in intensity. This he accounted for by suggesting that a condensation of the particles might take place so that the light would be dispersed according to the ordinary laws of reflexion from large particles instead of the laws which relate the dispersion of light by small particles. One more consideration which he wished to mention was that it had often been remarked that the corona was symmetrical with regard to an axis, which has been described as the equator of the sun. The apparent inclination of the equator to the ecliptic varied from 7 degrees 15 minutes to nothing. During the eclipse of 1871 it was under 7 degrees, during that of 1870 it was nearly 6 degrees, and during the eclipse visible at the Cape of Good Hope it was about 4 degrees, and during the eclipse visible in America between 4 and 5 degrees. He had only had time to look slightly into the matter, and he found that the axis of symmetry did not agree with the sun's equator, and the corona appeared to be always larger on one side than on the other.

Mr. Ranyard: Whose determination of the position of the sun's axis did you take?

Dr. Schuster: I took the photographs of 1871, as given in the *Memoirs* of the Society; that is, I took the north point as given in the plates of Col. Tennant's photographs.

Mr. Marth: But how did you calculate the position of the sun's axis with regard to the north point?

Dr. Schuster: The sun's equator is inclined at an angle of

7 degrees 15 minutes to the ecliptic; that is how I calculated it. If you had to determine the position of the sun's equator, how would you do it? You would have done exactly as I did.

Mr. Ranyard: When the position of the north point is known it is very easy to calculate the position of the projection of the sun's axis upon the photographic plate, but if I have understood Dr. Schuster he has spoken of determining the position of the axis of symmetry of the corona relatively to the sun's axis with a much greater degree of accuracy than is, I think, possible. In order to determine the position of the sun's axis you must first obtain the position of the north point, or of some orienting line, as, for example, a line parallel to the equator of the heavens. In the case of the 1871 photographs, which are probably better oriented than any other photographs of the corona, Mr. Davis, Lord Lindsay's assistant, just before totality took a series of photographs of the solar crescent upon a plate, the clock was stopped and the telescope was allowed to stand still, an exposure was made, and then, after an interval of half a minute or so, another exposure was made, and so on, until there were some eight or ten pictures of the solar crescent upon the plate. The telescope having been carried on by the earth's motion in the mean time, it was assumed that a straight edge laid upon the plate so as to touch all these images of the sun would be parallel to the equator of the heavens. This operation I repeated several times, and having determined the inclination of the straight edge to the side of the plate, I assumed that a similar line drawn on the plates upon which the corona was photographed would be parallel to the projection of the equator of the heavens. Probably it would not be safe to speak of the error of such a determination as being less than 1° , but no other photographs have, as far as I am aware, been so exactly oriented. Next to the photographs of 1871 probably the photographs of the 1869 eclipse were the best oriented, but the orientation of the other eclipse photographs was very doubtful. Probably there were errors amounting to five and perhaps ten degrees. The photographs of the 1875 eclipse recently published by Dr. Schuster in the *Philosophical Transactions*, were not oriented at all, and no orienting lines were given from which the position of the sun's axis could be determined. But supposing the position of the projection of the sun's axis accurately laid down upon the plate, there would still be a difficulty in determining with accuracy the axis of symmetry of the corona: there would probably be an error of say some 5° either way in laying it down.

With regard to the area of maximum polarization, which Dr. Schuster has referred to, I have given in the part of the eclipse volume which has been lying on the library table for the past

year, a list of the observations which tend to show that there is such an area. The fact, I think, cannot be doubted, though according to my observations it was not very marked during the last eclipse. Besides the explanation of the decrease of polarization suggested by Dr. Schuster, there is another and I think simpler explanation of the phenomena observed which I have suggested in the eclipse volume.* The particles in the lower part of the corona below the area of maximum polarization may be incandescent and emit unpolarized light, which would tend, together with the light from the incandescent gas, which we know is present, to drown the dispersed solar light. With regard to the formula expressing the intensity of polarization, set out by Dr. Schuster, he has not, I think, taken into account the polarization of all the elements which are seen superposed on looking at any part of the corona. It of course makes the problem much more complicated, but I have made certain assumptions with regard to the even distribution of the matter of the corona, &c., and have performed the integration and shown that the polarization should increase regularly outwards. The problem is not very elegantly treated in the eclipse volume, and I should like to see it more elegantly treated by some one else.

Capt. Noble said that we had heard a great deal lately about the corona visible during the recent eclipse being ten or twenty

*[Note by Mr. Ranyard.] The explanation suggested is that the light dispersed by our atmosphere tends to drown the polarization of the outer corona. Though the polarization of the light dispersed by small particles should increase rapidly as we pass outward from the sun's limb, yet the intensity of the light of the corona decreases very rapidly, and the area of maximum polarization will correspond to the place where the polarization of the light derived from the intervening sky and corona taken together is greatest. The observations show that though the light dispersed by the atmosphere during totality is itself polarized, yet the area of maximum polarization lies well within the limits of the visible corona. These observations are more especially interesting as they tend to show that—

The light derived from incandescent gas, taken together with the light derived from incandescent particles, in the lower corona (if such particles are present, as I think other observations seem to render probable) cannot bear a large proportion to the dispersed solar light. For we know that the light from the incandescent gas increases in intensity as we approach the sun's limb, and it seems reasonable to suppose that the light due to incandescent particles would also increase in the same manner; and since such light is unpolarized, it would, if it bore any large proportion to the total light derived from the lower parts of the corona, obliterate the area of maximum polarization, and cause the observed polarization to decrease regularly in intensity as we pass from the outer limits of the corona inwards to the limb of the moon—since this was not the case in the coronas visible during the eclipses of 1860, 1871 and 1878—we may conclude that on all these occasions the nonochromatic images of the lower parts of the corona must have been *far* too faint to photograph.

times less bright than it was in 1870 and 1871. He wished, before the question of eclipses was dismissed, to ask Mr. Penrose whether he thought that the corona visible on this occasion was twenty times less bright than in 1870?

Mr. Penrose replied that he could not personally give this information as, unfortunately, he had not seen the corona in 1870, but this he could say, that the general illumination on this occasion was much greater than in 1870. In 1870 he had found that it was difficult to read tolerably large print, whilst he could distinguish fine pencil lines with ease on the last occasion.

Mr. Ranyard read extracts from a communication from Prof. Piazzi Smyth *On measures of the lines of the B group in the spectrum of a high sun*. Prof. Piazzi Smyth had during the past summer gone to Spain for the purpose of observing the lines at the red end of the spectrum. He had given a diagram of the remarkable group of lines just below great B, and one of the chief matters of interest pointed out was that the B line itself, which is usually seen as a broad black line, was shown as consisting of a close group of fine dark lines, which he compared to a bundle of rods carried by a Roman licitor.

Mr. Ranyard also gave a short account of an important catalogue of double stars, made by Mr. Burnham, which the Council has decided shall be published in the *Memoirs*. It contains a list of the new double stars which have been discovered by Mr. Burnham with the Chicago 18½-inch refractor. Many of them are very close doubles, some 50 being within 1" of arc.

Mr. Edmund Neison read a paper *On Newcomb's corrections of Hansen's value of the secular acceleration*. He said that there was a discrepancy between the observed and theoretical value of the secular acceleration which was usually accounted for by assuming that the friction of the tides slowly stopped the rotation of the earth and so lengthened the day. In a recent volume Professor Newcomb had carefully computed a very large number of ancient observations of the moon, and in looking over this volume it had struck him that Prof. Newcomb had given undue weight to the very earliest eclipse on record though it was discordant with the others, whereas if this eclipse were left out of account the other eclipses gave a very much smaller value for the secular acceleration, and the observations agreed much better with theory.

Mr. Ranyard read a note *On the presence of particles of iron in the atmosphere*. He said that Professor Alex. Herschel had been much struck by a paper by Mr. John Murray, published in the proceedings of the Royal Society of Edinburgh, giving an account of his examination of the deep sea clays, which had been brought home by the Challenger expedition. In many of these clays

Mr. Murray had found small magnetic particles, which, on being extracted by means of a magnet, and moistened with an acid solution of sulphate of copper, showed the copper deposited in ramified spider lines, which were similar to the lines in which copper was deposited on the cut surface of a meteorite. From this and other circumstances Mr. Murray concluded that the iron particles had a cosmic origin. He believed that Professor Alex. Herschel had also examined the particles under the microscope, and was of the same opinion. Besides this, Dr. Nordenskjöld—a Swedish mineralogist—had collected the pure snow which fell during the latter part of a heavy storm in 1871, at Stockholm, and on melting it had found that the snow-water gave a sediment which contained particles of iron. Dr. Flight, of the British Museum, had also brought forward some experiments looking in the same direction, and a Swedish observer had exposed plates covered with glycerine to the wind, and caught iron particles. On his return voyage from America Mr. Ranyard thought that he would repeat this experiment at a distance of 1,000 miles from land, where it was not probable that particles of iron, carried up into the air from blast furnaces, would be present. He therefore exposed some glycerine plates on a wind-vane placed at the bow of the steamer. Some of these were exposed for thirty hours, some for eighteen hours. On one of the plates that was exposed for eighteen hours Mr. Ranyard found a small particle of iron rather less than the $\frac{1}{125}$ th or $\frac{1}{150}$ th of an inch in its longest diameter. He did not, however, feel satisfied with the experiment, as he thought the particle of iron might have been derived from the dust lying at the bottom of the box in which the glycerine plates were carried. He described the observation in hopes of inducing someone else to repeat it with still further precautions. The box in which the plates were carried had been given to him by Dr. Henry Draper; although it closed with a lid which would probably prevent dust getting in, yet it was possible that it had been lying about open, and on another occasion if he were repeating the experiment he would coat the inside of the box with glycerine, and keep it carefully closed until the plates could be examined under the microscope.

Mr. Gill asked whether the plate was exposed on a sailing vessel or steamer?

Mr. Ranyard replied it was on a steamer, and that the wind was not a head wind, but was blowing nearly at right angles to their course. He fancied such a particle might be carried by the wind, but it could not he thought have come from the funnel, which was a long way aft. It was a much larger particle than he had expected to find.

There was another matter he would like to bring before the meeting. Dr. Draper had intrusted him with a photograph of the solar spectrum compared with the spectrum of oxygen, to present to the Society. He would not hand it round, as it was valuable and might get broken. Mr. Christie had made a map of the part of the solar spectrum in the neighbourhood of the G line, and had found a number of lines not shown in the photographic print published with Dr. Draper's paper. If he understood Mr. Christie, he had, on examining the solar spectrum, found that the bright lines shown in the print were broken up, and he thought it improbable that the oxygen lines would be broken up in the same way. When in America, Mr. Ranyard had had an opportunity of examining this region of the solar spectrum with one of Mr. Rutherford's gratings of 17,000 lines to the inch. It was true that the bright lines of the solar spectrum shown in the photograph were very much broken up, but it did not follow that this would affect the matching of the bright lines with lines in the oxygen spectrum. The chief importance of the photograph which he held in his hand laid in the fact that the matching of the lines was shown in it much more beautifully than in the paper print which had been copied from it. There was one group of four sharp lines of different intensity, which matched very exactly not only in position but in relative intensity with a group of four lines in the oxygen spectrum. Dr. Draper was so struck with the coincidence of the group that he said he was willing to rest the whole case on these four lines. When you take into account what is the probability of two such groups of lines matching as they do, Mr. Ranyard thought that Dr. Draper was not putting the case too strongly in saying that it would rest entirely upon this one group. You could not estimate the chance of such central coincidences for an angle line, as, being less than eight or say only six to one, this would give a chance of more than 1,000 to one against four such coincidences, and then there was the coincidence in relative brightness to be taken into account.

Mr. Christie said that Mr. Ranyard had rather misunderstood his reasoning with regard to the question of the lines. The sole point upon which he had insisted was that whereas in Dr. Henry Draper's photograph the so-called bright lines were fuzzy and narrow, and the spaces between quite as broad as the lines in photographs they had taken and in Dr. Rutherford's five photographs of the spectrum, the dark lines (or the spaces between the bright lines) were narrow, and the interspaces bright and perfectly sharp, coming up to a sharp edge. That was the difficulty he had felt in accepting the view that these were really bright lines. With regard to the question of matching, he would

like to ask Mr. Ranyard, as he attached so much importance to the coincidence of four lines, how much importance he would attach to the coincidence of 65 lines? Prof. J. C. Draper had found a coincidence of 65 dark lines of oxygen, so that they were at present in a difficulty as to whether there were four bright lines of oxygen or 65 dark ones, and as far as the evidence which had been put before them went, Prof. J. C. Draper's 65 lines were depicted on a larger scale, so that they could determine the coincidence with a greater degree of accuracy. Dr. Henry Draper maintained that he had determined the coincidence with accuracy, but on the photograph exhibited the scale was too small to settle so nice a point. The matter of coincidence of these bright and dark lines was a very difficult one indeed. He could speak from a little experience. The slightest displacement of the spectroscope or the electrodes would apparently shift the lines, and it was difficult to answer for minute quantities in these coincidences.

Mr. Ranyard said that that made the fact of the matching the more remarkable, and it greatly increased the credit due to the man who could obtain a photograph showing a coincidence over a large area.

Mr. Christie said that the question was whether the lines did match.

Mr. Ranyard said that he did not know who Mr. J. C. Draper was, but he seemed to have discovered a coincidence of dark lines, which had escaped Bunsen and Kirchhoff, as well as the other distinguished physicists who had given attention to the solar spectrum, and had certainly looked out for the coincidence of dark lines with the lines of oxygen. But, supposing for a moment that the coincidence was fully made out, it would not disprove the coincidence of bright lines in the solar spectrum with oxygen lines. We are not in a position to assert that oxygen may not be present in the sun at two different levels, giving two different spectra. We know that several of the elements, such as carbon, aluminium, &c., give rise to different sets of spectral lines under different conditions, and it seemed very probable that oxygen might have a second or *allotropic* spectrum, for we already know it in the laboratory under an allotropic form as ozone. But he did not think that the case of Mr. J. C. Draper was at all satisfactorily made out. As far as he understood his paper the process which he had adopted for reading the wave-lengths of the dark lines in the solar spectrum was to enlarge a photograph of the solar spectrum by means of a magic-lantern apparatus, and he had compared the magnified image of the lines, which would no doubt be greatly distorted,

with a scale. The readings were then compared with the readings of the oxygen lines given in Watts' index of spectra. Now, if anyone would turn to the measures of the lines of the oxygen spectrum given in Watts they would find that there was a difference of two and sometimes three-tenths metres between the measures of Plucker and the measures given by Huggins. The question would not be settled adversely to Dr. Henry Draper until Mr. Christie or somebody else had photographed the oxygen spectrum, and shown the want of coincidence of the solar lines with the oxygen lines, in the same manner as Dr. Henry Draper had shown the coincidence.

Dr. Schuster said that Dr. J. C. Draper was a brother of Dr. Henry Draper, but he did not think that he had proved his point with regard to the coincidence of the oxygen lines with the dark lines.

Mr. Brett said they had all heard of some excellent photographs which Mr. Ranyard had taken. He would suggest that they should be shown to the meeting.

The President said that it would not be well to hand round such valuable negatives.

Mr. Ranyard said that the negatives were in Mr. Wesley's keeping, and anybody who was interested in the matter could see them after the meeting.

Mr. Marth said he would call attention to a phenomenon occurring next year, which, though it could not be observed by astronomers on earth, possessed perhaps as much interest as, say, a total eclipse of the sun observable only in some inaccessible region near one of the poles. The phenomenon in question would only be visible to astronomers on the planet Mars. On Nov. 12, 1879, about two minutes before 2h. G.M.T., a small black body would make its appearance on the south following side of the disk of the sun, in six minutes it would have fully entered upon the disk, and would proceed slowly from left to right in a direction little inclined to the ecliptic of Mars. About a quarter past 4h. another and bigger black body would encroach upon the disk and would occupy 21 minutes before it had fully entered upon it. The two bodies were the moon and the earth, and they would be visible from all parts of the planet where the sun was above the horizon. But observers placed along a certain zone or track would have the opportunity of seeing a third and apparently bigger body cross the sun's disk. It was at present somewhat uncertain at what time this third body would make its appearance, probably about a quarter to 4h., while the moon was yet alone on the sun's disk. This third body would come from the right hand side, in a direction at a slant of about 20°

towards the south ; at which point it would touch depended on the station, but Martial observers would have to be very quick to observe all the contacts, since the time for doing so would be limited at best to some twenty or thirty seconds. This third body was Phobos, the inner satellite, but Martial observers would be far more interested in the transit of the earth and moon than in that of Phobos, since in the course of a Martial year there were no less than about 1388 transits of Phobos across the sun visible from some part or other of the planet, while the number of transits of Deimos was about 133. On the other hand, transits of the earth and moon were rare occurrences, the last one having taken place in the year 1800, while the next to follow would happen in 1905. About a quarter to 10h. the moon, which meanwhile had apparently drawn nearer to the earth, would quit the sun's disk, and the last external contact of the earth with the disk would take place at Greenwich midnight. But before external contact, about half-past 11h., Phobos would again cross the sun's disk for certain stations, after having meanwhile performed a whole revolution round the planet. Mr. Marth made some further remarks, referring to the present near coincidence of the planes of the orbits of the two satellites with one another, and with the plane of the planet's equator. So far as our present knowledge of the orbits allowed an estimate to be made, the mutual inclination of the two orbits would amount, ten centuries hence, to about 50 degrees.

The meeting adjourned at a quarter past ten.

REVIEW.

Annals of the Astronomical Observatory of Harvard College. Vol. IV., Part II. Observations in right ascension of 505 stars. Printed from the Sturgis fund. Cambridge: Press of John Wilson & Son. pp. xix. 143.

"Within the past few years, it has become evident that, besides the fundamental stars (by which term is here understood the catalogue of 36 principal stars selected by Maskelyne), and the two principal polar stars α and δ Ursæ Minoris, positions of many others of very great precision are needed for the various uses of astronomy. The selection and observation of these additional half-fundamental or secondary stars have been differently made by different observers. The present series of right ascensions is intended as a modest contribution to this object." The stars (over 500) were observed with much persistency from 1862 to 1865 inclusive. Besides Mr. Safford himself, the author of this volume, Prof. A. Hall, Mr. W. A. Rogers, and Mr. A. McConnel took part in these observations, which were all chronographically registered. The epoch is 1865.0. The A. R. is given to thousands of seconds of time ; the declinations are mostly reliable to a tenth of a minute of arc. Especial regard has been paid to the region about the north pole. We find that some stars of the 5th and 6th magnitudes, which are not in the B. A. C. at all, have been observed. We have had occasion lately to review

briefly other volumes of annals of H. C. observatory, and did not expect so soon to have the gratification of seeing another like the present, which is a fine work in every way, and reflects on Mr. G. P. Bond, the director who planned the observations, and on Mr. Safford and the other gentlemen associated with him, the highest credit. The instrument employed was the east transit circle. These standard catalogues of stars are not only in various ways of the greatest utility at the time present, but if one day it shall be given to us to know something of the structure of the sidereal heavens, and the rationale of stellar motions, it will be to them, in conjunction with the spectroscope, that we shall be indebted for the solution of the grandest of problems.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

Sir,—Will you have the goodness to insert in the *Astronomical Register* the following explanation, by the Astronomer Royal, of the method employed by me in the observation of the spectrum of the corona in the solar eclipse of 1851. It was communicated by him to the *Athenæum* of October 26, with reference to a paragraph in Mr. Ranyard's note, "Does the gaseous part of the corona vary with sun-spots?" inserted in the preceding number, and reprinted in the *Register* for November.

Royal Observatory, Greenwich :

EDWIN DUNKIN.

1878, November 4.

THE ECLIPSE OF 1851.

In a communication by Mr. Ranyard to the *Athenæum* of October 19, page 501, last column, seventh line, it is stated that "Mr. Dunkin, during the eclipse of 1851, observed the eclipse through (a) glass prism without slit." This statement is incorrect, but no blame whatever attaches to Mr. Ranyard; the fault, such as it is, is entirely mine. Copy of my instructions to Mr. Dunkin is preserved at the Royal Observatory; the paragraphs which relate to the spectral observation of the corona are the following :—

"Endeavour to fix on the object-end of the telescope a piece of pasteboard (its plane parallel to the plane of the object-glass) with a narrow vertical chink, so that, when the telescope is pointed towards the sun, the light from the sun through the chink will pass to the eye-end of the tube, and a prism set up endways on the eye-end will be sure to receive the light.

"For an instant use the glass prism with the naked eye to view the corona-light through the pasteboard chink, and see whether there is anything peculiar in the colour."

These instructions were most strictly carried out by Mr. Dunkin.

The account printed in the *Memoirs* of the Royal Astronomical Society is a copy of the report rendered to me by Mr. Dunkin. Mr. Dunkin naturally omitted to cite the paragraphs of my instructions, with which I must necessarily be acquainted, but it would have been proper for me, in communicating his report to the Royal Astronomical Society, to have explained some of these details.

Mr. Dunkin's attention had been called by Prof. Langberg to a possible

irregularity of illumination in the green part of the spectrum; and from this originated the remark in the report, "green being certainly as bright as any other colour."

Royal Observatory, Greenwich:
1878, October 22.

G. B. AIRY.

EXPLANATION OF NAMES ON MAP OF THE MOON.

Sir,—I send a note on the "Explanation of Names on the Map of the Moon," which perhaps may be of interest to your readers. I would first point out an error which runs through the whole series, that is, that the letter "S" represents, not "*Schmiat*," but *Schröter*. I also presume to differ with some of the explanations given by Captain Noble in number 182 of your Journal, notably "*Eichstadt*," "*Rocca*," and "*Fra Mauro*."

I am unable at present to identify *Simpelius* and *Mösting*, but I trust that I may be soon able to add a note about them.

Observatory, Dun Echt:

I am, sir, your obedient servant,

November 2, 1878.

LINDSAY.

Asgaricus, was better known as the *Bishop of Bremen*.

Azout. Died 1691.

Alexander. Nothing to lead to identification. May be, 1. *Alexander the Great*; or 2. *Alexander Aphrodisiensis*, a commentator on Aristotle in the end of the 2nd century; or 3. *Alexander of Tralles*, a writer in the 6th century.

Blancanus. *Biancani Giuseppe*, born 1566, at Bologna. Died 1624, Parma. A learned Jesuit and prof. of math. at Parma. Published a treatise on the works of Aristotle in 1615, Bologna.

Blanchinus. *Bianchini Francesco*, born Verona, 1662; died Rome, 1729. Dr. of theology, librarian to Pope Alexander VIII. Author of "*Hesperii et Phosphori nova Phænomena*." Rome, 1728, fol.; in which he shows the method of mounting the long focus telescopes of Campani.

Barocius. *Barozzi Francesco*, born Venice, about 1530; died about 1590. Commentated on Proclus Diadochus and Plato.

Bettinus. *Bettini Mario*, born Bologna, 1735; died Bologna, 1786. Jesuit prof. of math. at Parma. Wrote on the philosophy of mathematics, and on Euclid.

Büsching. *Anton Frederick*, born ? 1724; died Berlin, 1793. Prof. of philosophy at Göttingen, and a well-known geographer. Published *Magazin für Historie und Geographie*, 22 vols. Hamburg, 1767, 1793.

Campanus. *Campano*. The translator of Euclid. Born at Novaro in the 13th century.

Cabeus. *Cabeo Niccolo*, born Ferrara, 1585; died Genoa, 1650. Jesuit and prof. of math. at Univ. Parma. Wrote on the magnet, 1629. Commentaries on Aristotle, &c.

Capuanus. *Capuano Francesco di Manfredonia* flourished about 1400. ? Neapolitan. Many commentaries of the theory of the planets of Purbach. Sacrobosco.

Cicchus. *Cecco d'Ascoli*, or more correctly *Francesco degli Stabili*, born about 1250, at Ascoli, died Florence, 1327. Prof. of astronomy at Bologna. He left a commentary on the *Sphæra* of Sacrobosco, Printed at Basle, 1485.

Crüger. *Peter*. Born Königsburg, 1580; died Danzig, 1639. Prof. of math. at Danzig, and was the teacher of Hevelius.

Cysatus. *Cysat Johann Bapt*, born 1588, Lucerne; died 1657, Lucerne. Was prof. of astronomy at Ingolstadt; wrote much on the comets of 1618, 1619, also made the first telescopic observations of a comet.

- Eichstadt, Lorenz*, born Stettin, 1596; died 1660, Danzig. Was prof. of math. at Danzig, published ephemerides of the planets from 1636—1650, and of the sun and moon from 1400 to 1800.
- Eimmart, Georg Christoph*, born Regensburg, 1638; died Nürnberg, 1705. An engraver on copper. In 1781, the heirs of Theoph. de Murr offered for sale 57 folio vols. of MSS. of Eimmart's, containing a most interesting collection of letters, &c., they eventually found their way to the Jesuit Coll. at Polotsk.
- Fra Mauro, Mattei Mauro da Firenze*, born 1496; died 1556. A Franciscan monk. Wrote on Sacrobosco.
- Gruemberger*. This should be read *Grienberger, Christoph*, born Hall, Tyrol, 1561; died Rome 1636. Jesuit prof. of mathematics at Rome.
- Hahn*. There are many of this name worthy of a place in the moon, but probably it was *Count Friedrich von Hahn*, of Mecklenburg. Born 1741; died 1805, who wrote much on observational astronomy.
- Hansteen, Christopher*, born Christiania, 1784. Famous for his researches in the theory of terrestrial magnetism; was director of the observatory at Christiania.
- Hausen, Christian August*, born Dresden, 1693; died Leipzig, 1743. Prof. of math. at Dresden. Wrote on the rotation of the sun. 1726.
- Hase, Johann Matthias*, born Augsburg, 1684; died Wittemberg. 1742. Prof. of mathematics at Wittemberg.
- Hommel, Johann*, born Memminger, 1518; died Leipzig, 1562. Prof. of math. at Leipzig. Left no writings. His pupil Scultetus was the teacher of Tycho Brahe.
- Kies, Johann*, born Tübingen, 1713; died Tübingen, 1781. Was mathematician to Prince Czartoryski at Warsaw, later astronomer at Berlin. Wrote much on astronomy.
- Lilius, Lilio Luigi*, born † Ciro; died 1576? Was occupied on the reformation of the calendar under Pope Gregorio XIII., his name is mentioned in the Bull of March 1, 1582.
- Manzinius, Manzini, Carlo Antonio*, born † and died (1677) at Bologna. Wrote on optics and astronomy.
- Milichius, Millich, Jacob*, born Freiberg, 1501; died Wittemberg, 1559. Prof. philos. and medicine at Wittemberg; tutor of Reinhold, commentator on Pliny.
- Moretus, Moret, Theodor*, born Antwerp, 1602; died 1667, Breslau. Jesuit, prof. of math. at Prague for 31 years.
- Pentland, Jos. Barclay*, 1800. Possibly this may be the man. He was a traveller in Bolivia and Peru, and wrote much on the Andes, and the mountains of Peru.
- Pitatus, Pitati, Pietro da Verona*. Middle of 16th century. A great composer of ephemerides, &c.
- Pontanus, Pontano, Gioviano*, born Spoleto, 1427; died Naples, 1503. Wrote a poem "Urania," publ. by Aldus, 1505.
- Rost, Johann Leonhardt*, born Nürnberg, 1688; died ditto, 1727. An amateur astronomer, wrote much.
- Santbech, Daniel*, of Noyon, about 1550. Edited the work on Triangles of Montregio.
- Sömmering, Samuel Thomas*, born Thorn, 1755; died Frankfurt, 1830. Was a celebrated surgeon, and prof. of anatomy at Mainz.
- Stiberius, Stüberl, Andrea*, lived about 1500, at Vienna, where he was prof. of mathematics.
- Ukert, Friedr. August*, born Eutin, 1780; died 1851, Gotha. Librarian and keeper of the medal cabinet at Gotha.
- Zagut, Zacutus, Abraham*. A celebrated Jewish professor of astronomy at Carthage.

- Messala, Macha Allah, or Messa Hala.* Jewish Arabian astronomer of the 8th century; his works were highly valued in the 14th century, and were printed at Nürnberg in 1549.
- Tarrantius, Tarutius, Lucius,* surname *Firmanus*. Philos. and math., born at Firmium in Italy; a contemporary and friend of Cicero and Varro.
- Tannerus, Tanner Adam.* A Jesuit, born Innsbruck, 1572; died 1632. Chancellor of the University of Prague, and author of the *Astrologia Sacra*. Ingolstadt, 1621.
- Segner.* Von Segner, *Johann Andreas* born Preshurg, 1704; died Halle, 1777. Prof. of physics at Jena, Göttingen, and Halle; a prolific and learned writer.
- Hainztlus, Hainzel, Paul.* Flourished end of 16th century; observed the Nova of 1572. See Tycho Vol. I. pp. 352 and 444.
- Furnerius, Furner, George.* A French Jesuit. Prof. of math., Paris; published valuable work on hydrography, Paris, 1643; was a correspondent of Riccioli on eclipses.
- Rook, Mts.* Called after Mr. Laurence Rooker, born 1623; died 1662. Gresham Prof. of astronomy; one of the promoters of the Royal Society, 1666.
- Schomberger, George.* A learned Jesuit, born at Innsbruck. Wrote on the sun and optics in 1622.
- Simpelius.* ?
- Sirsalis, Geronimo.* A Jesuit and friend of Riccioli. Lived 1651.
- Piton.* I believe Mr. Birt meant this for a Peak of Teneriffe, mentioned in Piazzi Smythe's "Experiment."
- William I. William Landgraf of Hesse,* the friend of Tycho Brahe. Lived 1570.
- Rocca, Angelo,* born at Rocca Contratè (Ancona), 1545; died ? 1620. Augustin Monk. Was librarian to Pope Sixtus X.; founded the Angelical Library at Rome, the first public library in that place.

COMET I. 1870 AND THE PERSEIDS.

Sir.—The probable connection of Comet I. 1870 with the *Perseids* referred to by Mr. Backhouse in the last number of the *Register* (and also pointed out by myself in the *Observatory* for September), leaves little doubt that the somewhat brilliant display of August meteors in 1871 belonged in part to this comet, and that had a strict watch been instituted on the 12th many more of these meteors might have been seen. The chief radiant on the few preceding nights was a few degrees N. of χ Persei, at $35^\circ + 59^\circ$, as determined by Prof. A. S. Herschel, with strong indication of other centres near η Persei and B Camelopardi.

The diffuse radiation often ascribed to the *Perseids* has been fully confirmed by recent observations. In addition to Prof. Herschel's shower at $35^\circ + 59^\circ$, there are others at $45^\circ + 59^\circ$ and $43^\circ + 53^\circ$, also an earlier radiant (July 30—Aug. 1) at $32^\circ + 53^\circ$, just S. of χ Persei. The meteors are all very similar and would seem to indicate a family of radiants belonging to the same system. This grouping together of contemporary and similar meteor streams is by no means an uncommon feature. In November there are two rich showers near the *Leonids*, one at $155^\circ + 36^\circ$, the other at $143^\circ + 28^\circ$, and the meteors cannot be distinguished from those of the great shower except by the direction of their paths. Mr. Corder, of Writtle, also just informs me that during a long watch on Oct. 22 last, he saw 35 *Orionids*, which indicated a triform radiant at $90^\circ + 14^\circ - 88^\circ + 24^\circ - 96^\circ + 19^\circ$, and suggests that the three centres really belong to one great stream or ring of concentric streams.

Ashley Down, Bristol:

November 7, 1878.

I am, sir, your obedient servant,

W. F. DENNING.

(From the *Times* of October 31, 1878.)**HEAT OF THE STARS.**To the Editor of *The Times*.

Sir,—In the admirable article on Edison's Tasimeter, in *The Times* of to-day, the writer seems to leave it to be inferred that it is only by the aid of the remarkable American invention that the heat of even the highest of the fixed stars can be observed.

In justice to English science I think it right to mention, though I should have preferred the notice to have come from some other hand, that, as the result of experiments conducted with the proper care and precautions, I was able to announce to the Royal Society in February, 1869, the detection of the heat of several stars.

The Government astronomer at the Cape of Good Hope, Mr. Stone, then first assistant at Greenwich, also successfully observed the heat of some stars, and further gave a rough estimate of the heating power of Arcturus and α Lyrae, in a communication to the Royal Society in January, 1870. He makes the amount of heat received from Arcturus, at an altitude of 25° , to be about equal to that of a three-inch cube containing boiling water at a distance of 400 yards; the heat from α Lyrae, at an altitude of 60° , to that of a similar cube at about 600 yards.

Upper Tulse Hill, S.W. :

Your obedient servant,

Oct. 29.

WILLIAM HUGGINS.

(Extract from *The Times* of November 4th, 1878.)**EDISON'S TASIMETER.**To the Editor of *The Times*.

Sir,—Dr. Huggins's letter on "The Heat of the Stars," taken together with the excellent article on Edison's Tasimeter published in *The Times*, would leave the reader to infer that the heat-measuring instruments in use before the invention of the tasimeter were not sufficiently delicate to detect the heat from the corona.

As long ago as 1842, Professor Magrini, of Milan, making use of an old-fashioned thermopile and galvanometer, found that the corona visible during the total eclipse of the 7th of July of that year gave so much heat that the needle of his galvanometer was thrown at right angles to its zero position, though with the same instrument he was unable to detect any heat from the full moon. An account of Magrini's observations will be found in the "Annuaire" of the Bureau des Longitudes for 1846.

In more recent times the Earl of Rosse has, by means of a thermopile placed in the focus of his great reflector, made a valuable series of measures of the heat derived from the moon at its different phases. These measures may be relied upon as having a comparatively small probable error. But from what I saw of the tasimeter during a recent visit to America, I am inclined to think that its extreme sensitiveness will be a bar to its use as an instrument which may be relied upon for quantitative results. The heat derived from the observer, and the heat due to currents of air, the motion of the instrument, and other unexplained causes, give rise to deflections of the needle which it is difficult to separate from the deflections due to the heat of the object under observation.

November 2, 1878.

Your obedient servant,

A. C. RANYARD.

Books received.—Total Solar Eclipse of July 29th, 1878. By Prof. H. Draper, M.D.—Six Months in Ascension. By Mrs. Gill. London: John Murray. 1878.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.	
		h.	m.		h.	m.	s.
Mon	16	15	3	☾ Moon's Last Quarter Sidereal Time at Mean Noon 17h. 39m. 58 ⁷ 58.	3rd Tr. I.	3	55
		14	13	Occultation of η Virginis (6)			6 9 ⁶
Tues	17	15	5	Reappearance of ditto Saturn at quadrature with the Sun			6 5 ⁷
Wed	18			Sun's Meridian Passage 3m. 9 ⁹ 9s. before Mean Noon	2nd Oc. D.	6	24
Thur	19				4th Tr. E.	6	52
Fri	20				2nd Tr. E.	4	14
					1st Oc. D.	4	53
					2nd Sh. E.	5	57
Sat	21	2		Conjunction of Moon and Mars 5° 1' N.	1st Tr. E.	4	26
					1st Sh. E.	5	16
Sun	22						5 46 ⁷
		9	24	● New Moon. Conjunction of Mars and Venus 1° 45' N.			
Mon	23	18		Conjunction of Moon and Mercury 4° 19' N.			5 42 ⁹
		18		Conjunction of Venus and Mercury 2° 29' N.			
Tues	24						5 39 ²
Wed	25	10		Inferior conjunction of Mercury and Sun			5 35 ⁴
		4	52	Near approach of 19 Capricorni (6)			
Thur	26	4		Conjunction of Moon and Jupiter 1° 13' S.			5 31 ⁶
Fri	27				2nd Tr. I.	4	6
					3rd Ec. R.	5	5 11
					2nd Sh. I.	5	36
Sat	28				1st Tr. I.	4	8
					1st Sh. I.	4	51
					1st Tr. E.	6	28
Sun	29				1st Ec. R.	4	24 16
Mon	30	6		Conjunction of Moon and Saturn 7° 14' S.			5 16 ⁶
				☾ Moon's First Quarter Saturn's Ring: Major axis = 38".77 Minor axis = 1".53			5 12 ⁹
Tues	31						
		1 ⁷	29				5 9 ²
Wed	1						

THE PLANETS FOR DECEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.			Declination.	Diameter.	Meridian Passage.	
		h.	m.	s.			h.	m.
Mercury ...	1st	17	54	6	S. 25 51	5".8	1	13.1
	9th	18	35	49	S. 25 7	6".8	1	23.2
	17th	18	52	6	S. 23 13½	8".4	1	8.9
	25th	{ 18 22 30 }	{ 18 16 39 }	{ S. 21 9½ }	{ S. 20 56½ }	{ 9".8 }	{ 0 }	{ 7.0 }
Venus ...	1st	16	31	5	S. 21 42	9".8	23	57.2
	9th	17	8	48	S. 23 2	9".8	23	46.3
	17th	17	52	37	S. 23 51½	9".8	0	8.7
	25th	{ 18 36 43 }	{ 18 36 43 }	{ S. 23 56 }	{ S. 23 56 }	{ 9".8 }	{ 0 }	{ 21.2 }
Saturn ...	1st	23	49	39	S. 3 44½	16".4	7	7.6
	9th	23	49	57	S. 3 41	16".2	6	36.5
	17th	23	50	39	S. 3 34½	16".0	6	5.7
	25th	23	51	46	S. 3 25	15".8	5	35.4
Neptune ...	3rd	2	23	9	N. 12 18	...	9	32.9
	19th	2	21	58	N. 12 13	...	8	28.8

Mercury sets an hour after the sun at the beginning of the month, the interval increasing till the middle of the month, after which the interval rapidly decreases.

Venus is too close to the sun to be well observed.

Saturn sets a few minutes after midnight on the 1st. On the last day of the year he sets an hour before midnight.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN
DECEMBER, 1878.

BY W. R. BIRT, F.R.A.S., P.S.S.

HYGINUS. The publication in the last number of the *Astronomical Register*, p. 268, of a sketch of the region N.W. of Hyginus, will greatly assist the reader in identifying the objects to which we particularly desire to call the attention of observers. *Observational confirmation* of the existence of the following objects is very important. In alluding to them we shall quote the designations given to them in "Lunar Chart, No. I," issued by the Selenographical Society. The first object is the craterlet on the north rim of Hyginus; it occurs both on the sketch and chart, but undesignated. In Schmidt's map it is larger and outside the rim. The second object is the "Spiral Mountain," which in both sketch and chart has the appearance of a double circumvallation; it has also this appearance in a drawing by Capt. Noble, taken on November 2, 1878, from 8h. to 9h., who says "it is a curious formation, consisting principally of a dome-shaped mountain, which might very well be taken for a crater were it not for the direction of its shadows. South-east of this dome-shaped mountain Capt. Noble found a brilliant valley, which appears to answer to ψ of the chart and to t of the sketch. It is this valley which Klein considers to be a recent formation. The Spiral Mountain contains on the south, between the inner and outer walls, two craterlets marked e and w on the chart, and on the outer wall on the north a craterlet marked g .

South-west of the Spiral or dome-shaped mountain are two craters, which form the eastern and western points of a nearly equilateral triangle, and are marked *x* and *s* in the chart; the craterlet to the south is marked *y*. On the sketch Klein's new crater is placed nearly in a line with *s* and *y*, but sufficiently close with *s* to form with *x* the base of an isosceles triangle of which *s* is the apex. The configuration of these objects on the chart is different, but the sketch on p. 268 is quite sufficient to point out the object; at the same time we shall be glad to receive drawings of the configuration of *x*, *y*, *s*, and the new crater.

DARK SPOTS NEAR HYGINUS. Miss Ashley has observed three dark spots, one south of Hyginus, one west of Hyginus south of the rill, and one west of Hyginus north of the rill. These have been long known, they occur in the Register as I A η 4, I A η 7, and I A η 8. Two were observed by Capt. Noble on November 2, 1878, viz., I A η 4, and I A η 7. Only one dark spot is given in Chart No. L, the sketch has two.

9, Grove Crescent, Stratford, London, E.

November 16, 1878.

THE SATELLITES OF SATURN.

Major and minor semi-axes of the apparent orbits of the satellites, expressed in semi-diameters of Saturn's equator.

		Mimas.	Encel.	Tethys.	Dione.	Rhea.	Titan.	Iapetus
Major		3.14	4.03	4.99	6.40	8.93	20.71	60.34
Minor	Dec. 1.	0.09	0.12	0.15	0.19	0.27	0.56	8.29
	„ 31.	0.12	0.16	0.20	0.25	0.35	0.75	8.51

The position angles of the axes of the apparent orbit of Titan are $0^{\circ}57'$, and those of the orbit of Iapetus on Dec. 1, $12^{\circ}09'$, and on Dec. 31, $12^{\circ}19'$ less than those of the orbits of the inner satellites.

Approximate Greenwich times of some of the conjunctions of the satellites with the ends of the ring, etc., v. p. 189:—

1878.	Gr. M. Time.			1878.	Gr. M. Time.		
		h.				h.	
Dec. 1	7.4	Dione.	e.	Dec. 6	9.9	Encel.	sp.
	9.9	Encel.	e.	7	6.3	Mimas.	e.
	11.9	Tethys.	nf.		7.3	Dione.	nf.
2	7.3	Encel.	sp.		8.9	Encel.	np.
	10.6	Tethys.	sp.		11.0	Tethys.	np.
3	4.7	Dione.	sp.	8	6.3	Encel.	e.
	6.3	Encel.	np.		9.7	Tethys.	sf.
	7.4	Titan.	sf.	9	8.3	Tethys.	np.
	9.3	Tethys.	nf.	10	7.0	Tethys.	sf.
	11.2	Encel.	w.		7.7	Encel.	w.
4	7.9	Tethys.	sp.		9.9	Dione.	np.
	8.6	Encel.	nf.	11	4	Titan.	nf. limb.
	8.7	Rhea.	np.		5.0	Encel.	nf.
5	6.6	Tethys.	nf.		5.6	Tethys.	np.
	7.2	Rhea.	w.		9.8	Dione.	sp.
	7.6	Encel.	sf.		9.3	Titan	np. limb
	9.0	Mimas.	e.	12	6.2	Dione.	e.
	10.0	Dione.	w.		9.0	Encel.	e.
6	5.0	Encel.	w.	13	6.4	Encel.	sp.
	5.2	Tethys.	sp.		9.3	Mimas.	w.
	5.6	Rhea.	sp.		9.6	Rhea.	np.
	6.4	Dione.	sf.	14	5.4	Encel.	np.
	7.6	Mimas.	e.		7.9	Mimas.	w.

1878.	Gr.	M.	Time.	1878.	Gr.	M.	Time.
			h.				b.
Dec. 14	8.1	Rhea.	w.	Dec. 21	7.8	Tethys.	sp.
	9.4	Tethys.	e.	22	6.4	Tethys.	nf.
15	6.5	Rhea.	sp.		8.2	Mimas.	e.
	6.6	Mimas.	w.		8.7	Dione.	sp.
	7.7	Encel.	nf.	23	5.0	Dione.	e.
	8.0	Tethys.	w.		5.1	Tethys.	sp.
16	5.2	Mimas.	w.		6.9	Mimas.	e.
	6.7	Tethys.	e.		8.1	Encel.	e.
	6.7	Encel.	sf.		9.0	Rhea.	w.
	8.8	Dione.	w.	24	5.5	Mimas.	e.
17	5.2	Dione.	sf.		5.5	Encel.	sp.
	5.4	Tethys.	w.		7.5	Rhea.	sp.
	9.1	Encel.	sp.	26	6.9	Encel.	nf.
18	4.0	Tethys.	e.		8.2	Tethys.	np.
	6.1	Dione.	nf.	27	5.9	Encel.	sf.
	8.1	Encel.	np.		6.9	Tethys.	sf.
19	5.5	Encel.	e.		7.6	Dione.	w.
	6.1	Titan.	sf.		9	Titan.	np. limb.
20	4.3	Rhea.	sf.	28	5.5	Tethys.	np.
	9.1	Tethys.	nf.	29	4.9	Dione.	nf.
	9.4	Encel.	sf.		5.2	Rhea.	sf.
21	6.8	Encel.	w.		7.2	Encel.	np.
	7.7	Dione.	np.				

Iapetus reaches its greatest eastern elongation on Dec. 9, and its inferior conjunction, north of the planet, on Dec. 29. A. M.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Sept., 1878.	To Dec., 1878.	To March, 1879.
Aldam, W.	Squire, H.	Jones, W. E.
Ryle, Rev. Canon.	Ward, I. M.	Rivas, Miss.
To Oct., 1878.	To Dec., 1878.	To Dec., 1879.
Lawton, W.		Green, G. W.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will at once inform him of the fact.

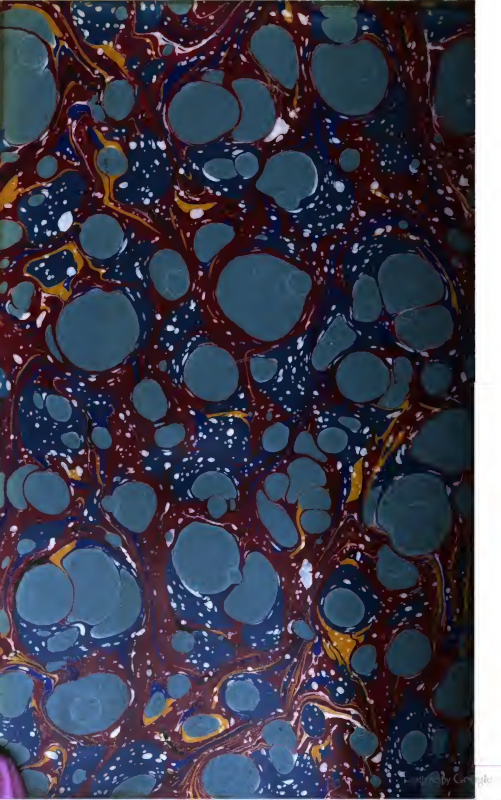
All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or penny postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for the Editor are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The *Astronomical Register* is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, payable in advance, by Penny postage stamps or otherwise. Subscribers in America may remit Four One Dollar Notes in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, Angel Court, Throgmorton Street, E.C., not later than the 20th of the Month.



UNIVERSITY OF MICHIGAN



3 9015 06541 3612

